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ABSTRACT

Contained are articles on three main aspects of science and technology. Part I contains articles related to the "Socio-Economic Impact of Science and Technology." The articles in Part II are grouped under a broad title, "The Structure, Function and Responsibilities of the Executive and Legislative Bodies in the Development of Science Policy." This section has articles on topics such as "Scientific Advice for Congress," "National Planning for Medical Research," and "Some Current Problems of Government Science Policy." Part III is entitled "Science and Academia." Some of the articles in this section are on topics such as "The Scientist as Public Administrator," "The Fiscal Dilemma of Academic Science," and "Educating for the Scientific Age." A bibliography of selected readings is included in the appendix. (PS)

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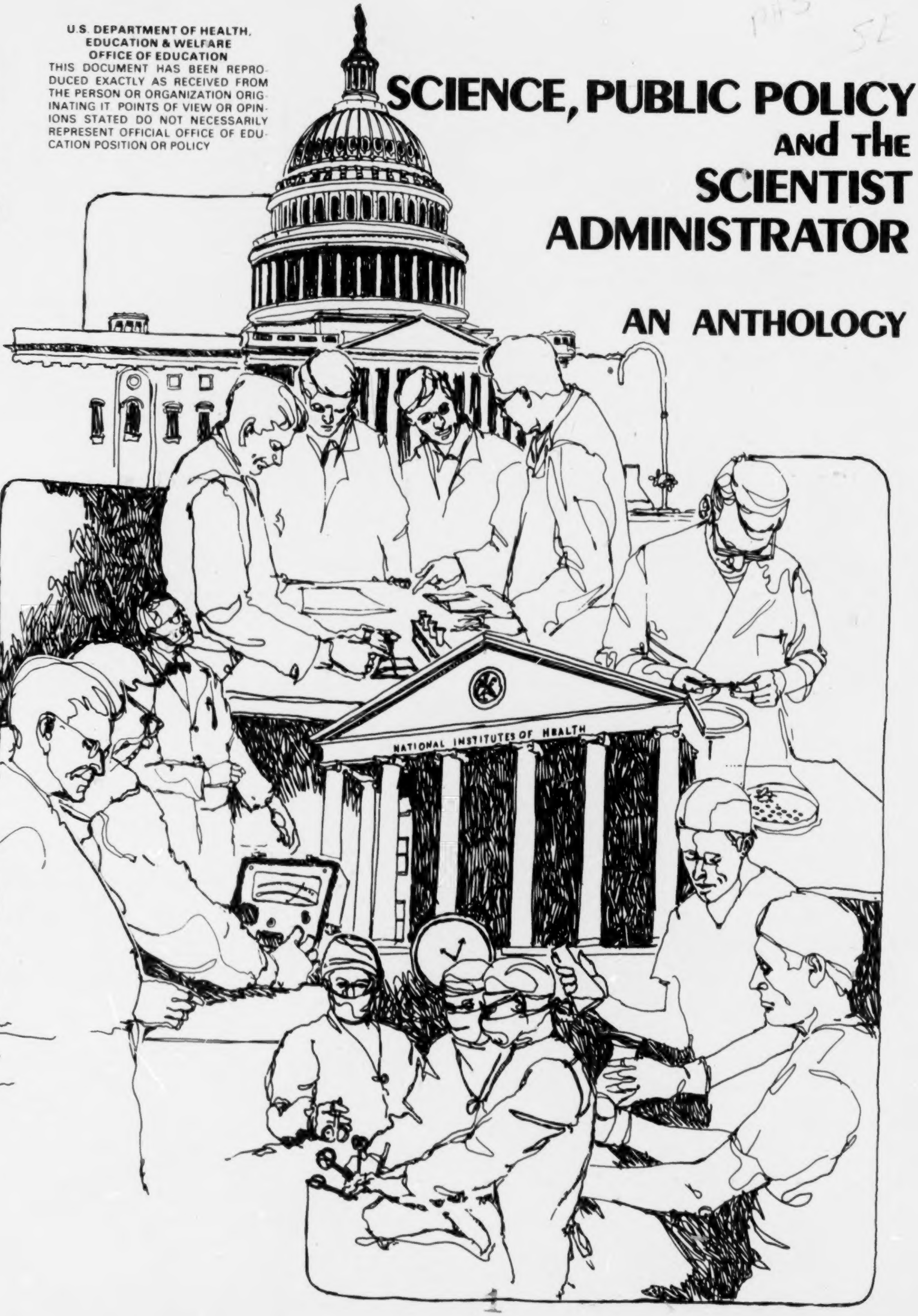
SCIENCE, PUBLIC POLICY AND THE SCIENTIST ADMINISTRATOR

AN ANTHOLOGY



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ED 068276

SCIENCE, PUBLIC POLICY
AND THE
SCIENTIST ADMINISTRATOR

An Anthology

Introductory Remarks

This anthology is the first of what is hoped will be a series of publications dealing with science and public policy which will become a part of the personal library of scientist administrators. It is clearly understood that future volumes will benefit from the results of this initial effort and to that extent the reader is urged to offer suggestions which may contribute to the content as well as the purpose for which it is intended.

The Staff Training—Extramural Programs (STEP) Committee, through this and other activities such as the seminar series, is attempting to provide new meaning to the responsibilities of the scientist administrator. The efforts of the Committee can only be judged by the degree of acceptance by the scientist administrators themselves. We look forward with confidence to the reaction to the initiation by STEP of this anthology.

RONALD W. LAMONT-HAVERS, M.D.
Associate Director for Extramural
Research and Training
National Institutes of Health

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If to an increasing degree we have the security of sound public opinion, if the extravagances and diatribes of political appeals fail of their object, and if, notwithstanding the apparent confusion and welter of our life, we are able to find a steadiness of purpose and a quiet dominating intelligence, it is largely because of the multitude of our people who have been trained to a considerable extent in scientific method, who look for facts, who have cultivated the habit of inquiry and in a thousand callings face the tests of definite investigations. With scientific applications on every hand, the American people are daily winning their escape from the danger of being fooled.... We need your method in government; we need it in law-making and in law-administering. We need your interest in knowledge for its own sake; the self-sacrificing ardor of your leaders; your ceaseless search for truth; your distrust of phrases and catchwords; your rejection of every plausible counterfeit; your willingness to discard every disproved theory however honored by tradition, while you jealously conserve every gain of the past against madcap assault; your quiet temper, and, above all, your faith in humanity and your zeal to promote the social welfare. We need your horizon; your outlook on the world.

Charles E. Hughes

Foreword

The selected readings contained in this compendium have been derived from many sources. They represent a small fraction of the literature that has appeared in recent years that deals with science policy and science administration. The purpose of this anthology is two-fold: first, it is intended to provide the reader with an understanding and at times a critical view of the many, and indeed, complex issues affecting science and public policy. Secondly, it should provide a glimpse into those personalities and forces, both within and out of government, that bear directly on these issues. It is the hope of the STEP Committee to provide an opportunity for stimulation of the intellectual curiosity that transcends the decisions and policies that affect our work-a-day world. This is a vital ingredient of "professional enrichment," which, of couse, is fundamental to the mandate of this Committee.

One note concerning the planning of this volume: The selections have been arranged to focus a perspective on their substance. The articles have been segregated into three sections, each dealing with a particular aspect of the over-all subject. Within each section, the articles are arranged to unfold an array of events which at some point, historically, have had an impact on the evolution of science in the public arena. Part I deals with the socio-economic implications and impact of science and technology upon society; Part II, the effect of national advisory and policy groups on science strategy within the Executive and Legislative branches of government; and Part III presents considerations of the fiscal dilemma of academia and science and the Federal support of biomedical research, including speculations on future trends. Because the selections do not treat the subject exhaustively, the bibliography, which appear in the Appendix, is intended to fill in the many gaps.

It is hoped that this anthology will become a part of your personal library. Together with the seminar and other activities of the STEP Committee, it is aimed at broadening the definition of that somewhat elusive term, science-administrator. Such an administrator is, to a degree, a hybrid who leaves the laboratory and its concerns with the specifics of science and becomes involved with the generalities of science as a public administrator. The scientist-administrator provides a vital link between the scientific community and the political decision makers. It has been said that basic research is judged to contribute by enlarging human understanding, applied research by enlarging options, and technology by putting selected approaches to work to create beneficial structures in systems. The scientist-administrator must be capable of moving freely and confidently in each of these areas at any one time, as the situation demands.

If it can be said that this anthology has contributed in some measure by providing new meaning to the responsibilities of the scientist-administrator, an increased depth of understanding of the political decision-making process, and an appreciation for the implementation and administration of those decisions, then it will have gone a long way toward achieving its goal.

ANTHONY M. BRUNO, M.D.
Chairman, Committee on
Staff Training-Extramural Programs
1970–71

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SCIENCE, SCIENTISTS, AND POLITICS

SCIENCE, SCIENTISTS, AND POLITICS *is made up of some of the papers presented at a conference on the role and responsibilities of science executives in the service of government. The conference, which was held in Santa Barbara, was sponsored by the Center in cooperation with the Twelfth Region of the United States Civil Service Commission.* ROBERT M. HUTCHINS *is President of the Fund for the Republic. His career as President and Chancellor of the University of Chicago provided first-hand experience with the subject of this paper.* SCOTT BUCHANAN, *Consultant to the Center, was Dean of St. John's College. His books include* Poetry and Mathematics, *reissued in paper-back last year.* DONALD N. MICHAEL, *author of the Center pamphlet,* Cybernation: The Silent Conquest, *is Director of the Peace Research Institute in Washington, D. C.* CHALMERS SHERWIN *is Vice President and General Manager of the Laboratories Division of Aerospace Corporation in Los Angeles.* JAMES REAL, *management consultant for government and industry, is co-author of the newly published Center book,* The Abolition of War. LYNN WHITE, JR., *former President of Mills College, is Professor of History at the University of California at Los Angeles.*

Robert M. Hutchins

I do not know much about science, but I know a lot about scientists. Though I do not know much about professional politics, I know a lot about academic politics—and that is the worst kind. Woodrow Wilson said that Washington was a snap after Princeton. Not only is academic politics the worst kind of politics, but scientists are the worst kind of academic politicians.

I wish at the outset to repudiate C. P. Snow, who intimates in one of his books that scientists should be entrusted with the world because they are a little bit better than other people. My view, based on long and painful observation, is that professors are somewhat worse than other people, and that scientists are somewhat worse than other professors. Let me demonstrate that these propositions are self-evidently true.

The foundation of morality in our society is a desire to protect one's reputation. A professor's reputation depends entirely upon his books and his articles in learned journals. The narrower the field in which a man must tell the truth, the wider is the area in which he is free to lie. This is one of the advantages of specialization. C. P. Snow was right about the morality of the man of science within his profession. There have been very few scientific frauds. This is because a scientist would be a fool to commit a scientific fraud when he can commit frauds every day on his wife, his associates, the president of his university, and the grocer. Administrators, politicians (not campaigning), and butchers are all likely to be more virtuous than professors, not because they want to be, but because they have to be.

One odd confirmatory fact is that those whose business it is to lie, such as advertising men, are often scrupulously honest in their private lives. For example, Senator William Benton, founder of the firm of Benton and Bowles, used to say that he had to be honest on Madison Avenue because if he wasn't word would get around that Benton was a crook and he would be ruined. When he retired from the advertising business he became vice president of the University of Chicago, whereupon he was prompted to say, "Look at these professors. What harm would it do them if word got around that they were crooks? They are all on permanent tenure!"

The general moral tone of academic life was once handsomely demonstrated at a University of Chicago faculty meeting. It was a solemn occasion. Two hundred full professors had assembled to discuss whether the bachelor's degree should be relocated at the end of the sophomore year, giving it and other degrees a meaning they had never had before. The faculty debated this proposition for two hours without ever mentioning education. The whole discourse concerned the effect of the proposed change on public relations and revenue. Mr. Benton, fresh from Madison Avenue, stormed out of the assembly shouting, "This is the most sordid meeting I ever attended in my life!"

There are many examples of this kind of professional morality. The chairman of a scientific department of the University of Chicago marched into my office one day and told me that we could not appoint one of the world's leading theoretical astronomers because he was an Indian, and black. Another faculty member, a great American sociologist, who was president of the American Statistical Association and president of the American Sociological Association, once informed me that it would be impossible to appoint a Negro to the faculty because all the graduate students would leave. We appointed the Negro anyway. As far as I know, no graduate students left.

The University of Chicago medical school violently resisted admitting Negro students. Negroes and Jews who had noncommittal names and were not otherwise visible to the naked eye were detected in photographs required with applications for admission. It took an executive order from my office to eliminate this requirement. Fortunately the medical school did not know that under the statutes of the University I had no power to issue such an order.

It is clear that the behavior of professors is questionable at best. Scientists are worse than other professors because they have special problems. One of these is that their productive lives often end at thirty-five. I knew an astronomer who was contributing to the international journals at the age of eleven. Compare that with the difficulty of contributing at a similar age to an international journal on, let us say, Greek law. A scientist has a limited education. He labors on the topic of his dissertation, wins the Nobel prize by the time he is thirty-five, and suddenly has nothing to do. He has no general ideas, and while he was pursuing his specialization science has gone past him. He has no alternative but to spend the rest of his life making a nuisance of himself.

Scientists are the victims of an education and a way of academic life created by their misinterpreters and propagandists. These misinterpreters have propagandized an entirely inconsecutive chain of consecutive propositions: The pursuit of truth, they say, is the collection of facts. Facts can be experimentally verified. Thus, the only method of seeking truth is the scientific method. The only knowledge is scientific knowledge, and anything else is guesswork or superstition. So Lord Rutherford could say to Samuel Alexander, the great English philosopher, "What is it that you have been saying all your life, Alexander? Hot air. Nothing but hot air."

A recollection I shall always cherish of one of our leading mathematicians, now a professor at Chicago, affords a stunning example of the frame of mind the propagandists have created. He came to Chicago as a graduate student. Toward the close of his first year I asked the chairman of the mathematics department how the boy was doing. "Oh, Mr. Hutchins," he said, "he's a fine mathematician, but I'm sorry to have to tell you, he's crazy." I said, "What do you mean 'crazy'? How does he evidence this unfortunate condition?" And the professor responded, "He's interested in philosophy!"

The misinterpreters' and propagandists' doctrine has paralyzing educational repercussions. According to its tenets, education consists in cramming the student with facts. There is not enough time to stuff in all the facts. Therefore, facts outside a narrow area of specialization must be excluded. One of our Consultants to the Center has described the education in science in the state university from which he graduated as two years of German, two years of military training, and all the rest mathematics, physics, and chemistry.

Seduced by the fact formula, the medical school at the University of Chicago set out on a perfectly sincere, although somewhat misguided, campaign against liberal education. There are countless facts in medicine. A medical school must fill its students with these facts or they will fall behind. This meant that there was no time to teach anything else. The medical school strongly recommended that the whole freshman and sophomore years be abolished–the junior and senior years had already gone–and that the entire curriculum be devoted to science and medicine. I can conscientiously say that any senior in the University of Chicago medical school knew more facts about medicine than any professor in a German university.

The consequences of this line of educational endeavor are clear enough. Everybody specializes. There can be no academic community because scientists cannot talk to one another. The chairman of the anatomy department of the University of Chicago brought this home to me once when we were discussing the great biological symposium that had been held to celebrate the University's fiftieth anniversary. I said, "Tell me, how was it?" He said, "I didn't go." When I asked why not, he replied, "Well, there weren't any papers in my field." Scientists cannot talk to anyone else because there isn't anyone else worth talking to. Hence, university life offers no remedy for the defects of their education.

The propagandists and misinterpreters of science have set the tone for the whole learned world in the United States. Their slogan is, "If you can't count it, it doesn't count." The influence of this slogan is felt in literature, philosophy, languages, and of course in the social sciences. The most striking feature of social science today is the total absence of theory. Its greatest modern achievement is the public opinion poll. Social scientists can count, but cannot comprehend.

Those who live their lives without theory are technicians, or mechanics. As a result there is no significant contemporary social science. Politics is viewed as power because power can be observed and measured. Power is something real. Therefore, using the misinterpreters' logic, it is *all* that is real about politics or political science. The most characteristic book title in social science in the past thirty years is *Politics: Who Gets What, When, How.*

In spite of the misinterpreters' nonsense, science contains elements of sense. Serious scientists know that science is just one very important way of looking at the world. When scientists are actually doing science they are caught in a great tradition. They know they are not simply collecting facts or conducting random experiments. No serious scientist believes that if a million monkeys were put down at a million typewriters one of them would eventually turn out *Hamlet*. Nor does he think the scientific method is the only method. Scientists do not use the scientific method outside of science.

How the propagandists and misinterpreters of science have managed to take over all the academic virtues and label them "scientific" escapes me. I ran across a fascinating study of the scientific attitude by a professor of education. This learned gentleman had written to sixteen eminent scientists and asked them what characterized the scientific attitude. These were the replies:

CHEMIST: *Openmindedness* . . . PHYSIOLOGIST: *Intellectual honesty* . . . BOTANIST: *Openmindedness* . . . ZOOLOGIST: *Observation, inquisitiveness, perseverance and industry, objectivity and critical independent reflection* . . . PHYSICIST: *Objectivity* . . . SOCIOLOGIST: *Objectivity* . . . MICROBIOLOGIST: *Respect observation* . . . MATHEMATICIAN: *Openmindedness* . . . ANTHROPOLOGIST: *Openmindedness* . . . CHEMIST: *Practiced willingness to label conclusions tentative until supported by reproducible or confirmed data* . . . AGRICULTURIST: *Desire to tolerantly explore ideas* . . . MATHEMATICIAN: *An open mind* . . . PHYSICIST: *A will to know the truth* . . . CHEMIST: *Insistence on critical examination* . . . DIRECTOR OF EDUCATIONAL RESEARCH: *Intellectual curiosity* . . . PSYCHOLOGIST: *An inquiring mind.*

Obviously this study shows that science has a corner on all the rational processes of thought.

But there is not an honest scholar in any field who would not insist on being openminded, honest, and objective, and on considering all the evidence before he reached a conclusion. You can hear Thomas Aquinas laughing.

The propagandists of science say, "Sure, but fellows like Thomas Aquinas had commitments. They all had philosophies and principles that distorted their thinking. Scientists haven't any." The answer to this is that everybody has a metaphysics. Every scientist, for example, has a commitment to the reality of the external world. The distortion comes when the metaphysics is denied instead of being recognized and made as rational as possible.

Understanding science is an indispensable part of a liberal education. To demonstrate my sincerity, I point out that at the University of Chicago one whole half of the first two years of every student's education was natural science. St. John's College, with which I also had something to do, is the only college in the United States that requires four years in the laboratory for every student. An education without science is no education at all. The limitations and possibilities of science cannot be understood without scientific training, and our very existence depends on comprehending these limits and possibilities.

We do not know what science is, and partly as a result we do not know what politics is. Mr. C. P. Snow is wrong about the two cultures. There is only one, and it is pseudo-scientific.

The leading phenomena of our time exhibit a curiously ambiguous character. Technology may blow us up, or it may usher in the paradise of which man has been dreaming ever since Adam and Eve got kicked out of the first one. Bureaucracy may stifle democracy or be the backbone of democratic government. Nationalism may disrupt the world or prove to be the necessary precondition of a world community.

Unfortunately these ambiguities do not lend themselves to scientific procedure. Our essential problem is what kind of people we want to be and what kind of world we want to have. Such questions cannot be solved by experiment and observation. But if we know what justice is, which is not a scientific matter, science and many other disciplines may help us get it.

The problems resulting from these ambiguities are not going to be solved by men of fractional or pseudo-culture. The solution depends on moral and intellectual virtues rather than on specialized knowledge. It is a humbling thought to recall that 25 per cent of the SS guards in Nazi Germany were holders of the doctor's degree.

The solution of these problems must lie in the reorganization of American education and in the redefinition of its purposes. A liberal education, including scientific education, must be established for all, and true intellectual communities must be built where men may overcome the limitations of their fractional cultures. This would require a drastic change in what the nation expects of American education, and an equally drastic alteration in the habits of academic people. I think it will be agreed that this cataclysm is not likely to occur in the lifetime of the youngest person reading this.

The immediate program, then, has to be something else. It must be an attempt to build intellectual communities outside the American educational system and to form widespread connections among the intellectual workers, using these communities as points of interconnection. The hope for the immediate future, as far as we have one, must rest in our capacity to communicate with the adult population. For one thing, unless we do, the rising generation may not have a chance to rise.

It is in centers like the Center for the Study of Democratic Institutions and in the multiplication of meetings like the one that produced these papers that we might get some help with the development of a real culture, and a real understanding of kinds of knowledge and the limits and potential of each kind. The radiation from these points might light the path to a just community for ourselves and for the world.

Scott Buchanan

The implication in discussing the nature of science and technology is that a distinction should be made between science and technology. Such a distinction is almost wholly unrecognized in our scientific cultural environment. In a recent seminar in which I participated the question of the difference between science and technology came up and the answer was: "There isn't any. We no longer separate them." This is a shocking statement. It is sobering to think that there is no possibility of distinction.

C. P. Snow has said that scientists and technologists have become soldiers. They are not working for themselves: they accept orders from others. They are not able to take responsibility for their own strategic judgments in science, to say nothing of the uses to which their work will be put. Whether the decisions are being made on the scientific or the technical level, scientists are not making them.

President Eisenhower in his farewell speech pointed out two things that needed to be watched: the hook-up among the military, the scientific community, and the industrial community, and the hook-up between the scientist and the administrator. We may have heard more about the scientist-soldier than about the scientist-manager, but the latter is equally threatening to the political community.

When a scientist is a soldier, he is subject to direction and is a means to an end established by someone else. When he is a manager, he sets the goals and directs other people. But this may not be as deep a paradox as it first appears. Both as a soldier and as a manager the scientist is involved in practice, in practical activity. He is working in what a traditional philosopher would call the "realm of practical reason." Usefulness is the standard by which he judges his work. Thus it is difficult to distinguish between science and technology because part of the meaning has gone out of science. The scientist has diminished not because he has become irrational, unreasonable, or arbitrary but because he has become a technologist.

Limiting science to the practical realm is comparatively new. Science was not born in the fifteenth or sixteenth century. The word "science" has had a long usage —about 3000 years—and until modern times its meaning contained concern about truth, pursued by speculative or theoretical reasoning rather than practical reasoning. These too are diminished words. Speculation has become something done on the stock market, and theoretical means "academic" to the general public. To the technical scientist theory is simply a means to an end. But there are some slightly old-fashioned scientists around who feel that the essential nature of science is not involved with practical reason. They say the scientist's work is to discover the truth, formulate it, and make it a matter of public as well as professional knowledge.

In Thorstein Veblen's striking phrase, "A scientist is addicted to the practice of idle curiosity." This defiant definition states in a humorous way a high dogma about what science is. This is the origin of the popular notion that the scientist is neutral on questions of utility or on the affairs of practical life. Idle curiosity means that the scientist is concerned only with truth. The results of the search for truth may be used for good or evil, but it is now said, even by scientists, that judgments about their use cannot be made by science.

If the scientist's concern is truth, it is his responsibility to be sure that science is not misused so that something false comes out of it. The burden of maintaining the activity of discovery implies a responsibility for academic freedom, but few scientists have defended academic freedom in this country though it has been in danger for the last generation. Perhaps it is because most scientists do not distinguish science from technology. Academic freedom may not be essential to questions of application and use. There is not much point in defending it if truth is not the object. If there is any absolute reason for academic freedom, it is that the search for knowledge of truth is an activity of human beings essential to everything else they do. The heaviest responsibility of the scientist to society may be to refuse to make himself useful.

Several kinds of sharply different judgments are to be made about the whole range of science and technology. The scientist, as a man concerned about the truth, makes one essential judgment about his findings: whether they are true or false. The technician, as an original inventor or as an adapter of something already discovered, makes a judgment of usefulness or fitness. He decides whether it works, and need not judge whether it is good or bad in any other sense. Business or industrial interests make different judgments from those of the scientist or technologist, which partly explains the difficulty of communication between the laboratory and the industrial manager. A much more general judgment about the utility, validity, and desirability of scientific work is made by society and imposed by social pressures.

But there is something missing in this series of judgments. The purposes of science may be considered by the scientist as a professional man. "Profession" as it was once understood meant more than a specialty. Universities were founded in Europe to educate and certify those who aspired to the professions, and the training included more than science. Students were taught the liberal arts, and achieved a realization of a larger theoretical, speculative body of knowledge in which the sciences are placed. From this point of view it is possible for a scientist to stand before the community and say "yes" or "no" to the alternative applications of science. But we no longer understand what the liberal arts are. We call them philosophy, but philosophers have shrunk into departmental academicians. The professional man, in fact the whole society, does not have a good philosophical background, and as a result there is a kind of judgment that is not being made. It is the only kind of judgment that could distinguish between science and technology.

Although medicine has lost a great deal of the philosophical professional integrity that was once expressed for an earlier time in the Hippocratic oath, physicians as individuals and as a group still make professional judgments. They do not prescribe poisons indiscriminately; they do not let commercial pharmacists dispense certain drugs without prescription; they judge malpractice. Although these judgments seem to belong to ethics, they are not primarily ethical. They are based on the professional theoretical knowledge of the physician. If the natural and social sciences wish to become professional, they need to discover and formulate such judgments both for themselves and for society. But in order to do that they will have to become philosophical enough to distinguish between truth and workability.

Donald N. Michael

Anthropologists and historians tell us that a crucial juncture in the life of a culture occurs when the assurance that it has gained from an unchallenged world view of values, goals, and logic confronts the unchallenged world view of another culture. It is not easy for men to change their view of the world, for it is part of their view of themselves. The challenge of other values threatens all that has given them comfort and support. It takes strong men and felicitous circumstances for a society to ride out the storm of contact with another culture and learn and grow anew.

It is by no means certain that this will happen. Some people are shattered by new experiences; so are some cultures. As segments of society splinter and converge, new institutions and new modes of thinking are generated. Some societies blossom in their revised form; others die.

Today we are faced with such a cultural crisis. The problems of making suitable policies for scientific work in the government arise chiefly from a profound cultural conflict. This conflict is the three-way confrontation among the scientific community, the non-scientific political governmental community, and the general public.

What is meant here by an adequate policy for federal science must be made clear at the outset. Such a policy would reconcile the needs of science and technology with the needs of the rest of society. Policy now springs from resolving disputes for priority among various projects. It is made in many places, from the Pentagon to the Department of Agriculture, as well as in those offices assigned part of the policy-making task. But nowhere do the social implications of science have a basic part in the formulation of policy.

Today, science and technology are not neutral. Not only does their development require vast social and human resources, but they are pursued because their powers for enhancing or degrading humanity are recognized. This non-neutrality demands an explicit relating of science and technology to the needs and processes of society. This relationship should be the foundation of federal science policy.

The one consensus among the three cultures—the scientific community, the non-scientific political community, and the public—is that the task of government is to serve the general public. There is no such agreement about the relationship of science to government and to the general public. There is no set of values mutually subscribed to by the three cultures that defines the proper purposes of science and technology and thereby the appropriate restraints and supports needed to fulfill those purposes. Nor is it clear that such a set of values can be deliberately produced. Values do not derive solely from rational considerations. They are historical products of emotion and plain accident as much as, or more than, reason. This is one weakness in the thesis that the scientific method by itself can solve society's problems.

Within each of the three cultures are men and institutions with different viewpoints and different goals. These dissimilarities are crucial. Some of them derive largely from training; some are induced by the preconceptions that each group has about the other two and about itself. Two of the three are contending for the power to insure that their particular values will prevail: the science community and the non-science governmental community. The general public has essentially no power.

The *science community* is represented at its upper levels by two types of scientists. The "traditional" type considers government to be synonymous with mediocrity and irrationality. These men feel that science must be left free to pursue its own ways. Their attitudes toward the rest of society are frequently ambivalent. They avoid involvement in social questions. Some of them perceive society as subject to, if not already operating along, logical lines. Others consider society as incorrigibly irrational and therefore unrelated to them. They are seldom asked to consider the social implications of their actions. By attending to their work, advising on the technical merits of this or that proposal, they can maintain the comfortable delusion that science can still be pursued without thought of the social consequences. Frequently they work for the university or for big industry, advancing the favorite programs of their employers.

Then there is the new breed of scientist around high Washington conference tables—the science entrepreneur, the "political" scientist. These men want to manage the bureaucracy to the extent necessary to make it behave the way they think it should. They have a sense of political technique, and they enjoy and seek power. Like the traditionalists, they feel that science is theirs, that no one else has the right to tamper with it. It is they who should decide which projects deserve emphasis. They believe a good dose of science would fix society fine, as C. P. Snow has so frequently tried to demonstrate. There are wise and modest men with social imagination in this subculture, but frequently the powerful members of this group are self-assured to the point of arrogance about their own abilities, about the over-riding rightness of scientific values and methods, and about the validity of their view of how society operates and what it needs.

The science entrepreneurs are supported by and in turn support big business, big publicity, big military, sometimes big academia and parts of big government. They are both the captives and the kings of these powerful coalitions – kings for obvious reasons, captives because in reaping the benefits of affiliation they capitulate in some degree to the operating principles of these institutions. They have climbed to power through conservative hierarchies and tend to hold conservative values. The infusion of émigrés from the disciplined institutions of Europe seems, in general, not to have been a liberalizing influence. The more powerful the "political" scientist gets, the more omnipresent he is at major deliberations on science policy.

The *non-scientific community* in Congress and the bureaucracies regards itself as the bones, meat, and brains of government and society. They resent the "woolly-headed" scientist who may be trying to change their ways or implying that these ways are inadequate. They are not about to be displaced by a new attitude or a new kind of knowledge. Scientific expertise is respected, but the political and social naïveté that is supposed to accompany it is regarded with disdain. A general feeling exists among these "non-scientists" that science must be controlled. Usurpation of power is feared, partly because of a conviction that science somehow cannot be stopped.

These men consider society a non-rational environment. They see the political process as subtle and changing, responsive to many pressures of which science is only one, and by no means the most important. They view science as a means, not as an end. But they are confused about means and ends in general, as well as about the implications of science, and have no clear view of the proper role of scientists in formulating policy.

These two cultures between them decide on national science programs. They are in deep conflict within and between themselves. There are great political and ethical splinterings in the science community alone. The entrepreneurs claim to speak for science, but speak only for their faction. The traditionalists are fearful and envious of the "political" scientists, upon whom they must depend for their survival, especially if they hope for accomplishment in fields requiring expensive equipment or team research. Both groups are dissatisfied with the workings of government.

Given this clash of cultures, how can a valid basis be found for policy-making in federal science? We must discover a common ground from which science and technology can be intelligently directed. We must be able to evaluate the social consequences of scientific innovation. We need to plan our economics to assure the effective and humane introduction of modern technologies. We must equip government to meet new regulatory and managerial tasks. It is not clear that these responsibilities can be met by any traditional form of government; nor is it certain that democracy can be preserved in doing so. What is clear is that we cannot continue to bumble along.

Already we are in desperate trouble over nuclear weapons. We are about to be overwhelmed by that terrible blessing of medical technology, overpopulation. The social implications of biological and psycho-pharmacological engineering are already evident. Cybernation is causing serious problems. What is more, our environment is being changed in ways no cybernetical system can cope with indefinitely. It must respond to a tremendous and growing range of information at increasing speed and with increasing accuracy. Instability of the system is the inevitable result.

In spite of these menacing developments we remain unable to forecast the social consequences of technology. This is partly because of the limited vision of both the non-scientists and the scientists. The first group does not have sufficient knowledge of technology to sense the potentialities of new developments and therefore cannot predict their social impact, and they are too preoccupied with conventional assessments of political issues and impacts. The second group is aware of the technological possibilities but is not sufficiently sensitive to their social implications. Some of the scientists care only about the success of their favorite projects. Some apply to these problems a personal pseudo-sociology made useless by its arrogance or naïveté. And still others dodge responsibility by arguing that technology itself is neither good nor bad, that its virtues are determined by its uses.

Another reason why the social repercussions of science are difficult to forecast is that we have too little understanding of the social processes. This limitation has been fostered by the disinclination of the natural scientist and the government operator to stimulate work in the social sciences. The bureaucrat feels threatened by the possibility that formalized knowledge will replace "experience" and "political know-how." Furthermore, the social sciences might demonstrate that the products of technology, or even science itself, need social control. This is an unhappy prospect for those scientists who are feeling for the first time the satisfactions of wielding power.

Since the consequences of scientific and technological developments are not fully predictable, it would seem impossible to establish priorities for individual projects on any sensible basis. Yet the forces of technological advance compel some kind of choice. Creative talent is a scarce resource, and the availability of money is a political, if not a real, limitation. "Political" scientists push their preferences vigorously, and the very existence of large programs influences selections in the absence of better criteria. Priority decisions today depend on political and economic pressures, personalities, and public relations.

The public relations juggernaut, in particular, imposes a crippling distortion on science and on those who would make scientific policy. From the laboratory to the launching pad science and technology are harried by promises about "product superiority" and the glamour of "breakthroughs." Commitments are quickly publicized and then science is pressed to maintain the "reality" of the commitments. The natural failures of science and the natural limits of accomplishment are covered by an ever-deepening layer of misrepresentation, deviousness, and downright lies. So pervasive becomes the aura of untruth that it is hard for anyone, from the man in the laboratory to the public, to know where reality lies.

A cliché of our political folklore is that somehow the *public* will make everything right. In its wisdom it will judge between the contending power groups, evaluate technologies, establish a scale for priorities. But the public, the third culture, hardly knows what is happening. Understanding or judging the conflicts and compromises now occurring between science and government is far beyond its capacity. The public is caught between a publicity-induced fantasy world where science knows all the answers and a frustrating actuality which it does not realize is caused at least in part by the inadequate or incorrect use of science and technology. The frustrations are blamed on someone else: Russia, the government, perhaps the intellectuals, seldom on science. The public still believes in the mad scientist working on bombs, or in the humble scientist laboring over polio vaccine. The member of government, civil servant or politician, is perceived no more realistically.

Rather than becoming able to resolve the problems of science policy, the public is likely to become increasingly alienated both from government and from science. As with many other groups in the past that have met cultures somehow superior to their own, the public may withdraw from the challenge of "adjusting up" to the new priests and the new power. How, in fact, can the ordinary citizen adjust up to a computer-run society and classified questions of life and death?

One segment of the public will not surrender without protest. This is the group of articulate, concerned laymen who are not solely scientists, politicians, or civil servants and who worry about the arms race, overpopulation, the ascendancy of the "political" scientist, and the inadequacy of non-scientific bureaucracies. These people might be the moderators, the synthesists, for a new culture. They do not have the trained incapacities of those solely immersed in the two contending cultures, and they do have perspective that the general public lacks. But these very characteristics may deny them the opportunity. The day of the technical specialist grows ever brighter. The scientist will not freely yield his newly gained power, nor will the government worker relinquish his long-held dominion. Neither is likely to give ground to a non-specialist who cannot build bombs or tread bureaucratic water, or otherwise play according to the rules of science and government.

The character of the coming generation of scientists is changing. The attributes attractive to laboratory directors interested in team-work are bringing a new personality into science. The old-guard traditionalists may be on the way out. Those who succeed will be those who are good at working with – or subverting – the non-scientific bureaucracy. Will these men be good scientists? This is not the important question. The real concern is for whom they will speak, and for what ends.

The problem in trying to resolve the ambitions of the two power cultures is that neither group has a clear view of what it wants in the way of policy for governmental science. As long as there is no community of values to guide judgment, basic policy decisions cannot be made, much less decisions on specific priorities for specific projects. Yet crises are arising on every hand. The evolution of a consensus cannot be awaited. If this society does not learn how to assimilate the changes that confront it, it will not survive.

Chalmers Sherwin

Science and technology are the key to the future, the key to power, and the key to the solution of the problems we face today. They alone will not save us, but if we seek to untangle our problems without them, we are lost.

In the last thirty years the increasing sophistication of the physical and biological sciences has exhibited the properties of a true revolution. It has radically altered the social organization within which it grew. It emerged in less than one working generation, and the suddenness of it caught all of us off balance. People still believe that science can be handled by the techniques and devices that it has itself made obsolete, or that if the problems it has brought are ignored they will vanish.

A common modern complaint is that while government has spread like an octopus, our problems have grown worse. It follows that the cure for our ills is less government. But government did not bloom spontaneously. It grew in response to the scientific revolution. As men have invented more gadgets and uncovered more knowledge about the world, an enormous expansion of government has been necessary, both to protect the public interest and to foster further scientific advance.

In 1800 the government of the United States played a modest role. It had an army, a postal department, a tax on whiskey, and some import duties; the Department of State kept track of the world. That was about it. But by 1830 railroad and steamboat traffic began to grow, and, to regulate it in the public interest, so did federal power. Later, internal combustion engines were invented, more was discovered about aeronautical science, and suddenly airways had to be regulated. Telegraph, radio, and television each generated complicated governmental problems. Modern chemistry and pharmaceuticals brought into being the whole field of food and drug control.

The economic disaster of agricultural overproduction, a triumph of applied science, is a prime example of the difficulties that technology has handed to government. The farm problem really began in 1862 when land-grant colleges were founded with federal support. By 1900 science was being applied to agriculture on a big scale, and by 1920 food production was beginning to be excessive. Hybrids, modern machinery, new methods of food processing, and new types of fertilizers were developed, and all at once America was producing too much food. Science and technology caused the surplus, but the federal government had to try to cope with it. Its efforts to do so, plus its efforts to make agriculture still more efficient, have spawned a giant bureaucratic structure.

The biggest surge of all in government growth was caused by the exploration of the atom. In 1939, when science suddenly found a major key to the secrets, no one but the government could afford to exploit it. Science has not stopped finding keys–those to space, for example – and the job of the federal government has not stopped getting bigger. Atomic and space research are unsuitable for private exploitation, not only because the government alone can afford the massive costs, but also because the results require governmental control.

The expansion of government suggests support for the idea that government should control science and technology. The feeling that modern knowledge and power must somehow be turned to the public good has currency. Even those interested only in the progress of science want government to help sustain its advance. Whether government's job is constraining science to serve the public interest or promoting the scientific front, or both, it must understand the phenomenon with which it is dealing.

Unfortunately, the people running government often do not understand science and technology. Despite some notable exceptions, scientific ignoramuses usually handle scientific decisions. The serious technical questions, such as how atomic energy and military space operations can be controlled, will remain unanswered until this basic difficulty is somehow solved.

Government managers of science and technology often do not know their business, partly because, as C. P. Snow argues, our educational system is no longer geared to the source of our power. Our power now rests on science, but we let those who administer and govern remain incompetent in the substantive knowledge of the area.

The revolution in science can be distinguished from the industrial revolution by the fact that a high school undergraduate can understand the principles of the latter. The steam engine, a railroad train, and, with a little more effort, even an electrical generator are within his grasp, but he gets lost in modern biochemistry, electronics, and nuclear physics. Mastery of this new knowledge is not quickly won. The subtleties of modern research and development, or even of technical production, are not easily learned late in life. But a manager *must* know the substance behind the problems he handles if he is to be effective. It is increasingly true that critical evaluation of substantive technical details is the very heart of policy decisions. The era of classical administrative formulation, "You name it, I'll manage it," is past. Today, few people except professional scientists have the technical sophistication necessary to make many of the crucial decisions affecting both science and society.

Using scientists in government seems an obvious answer to the dilemma of management. But creative scientists and engineers are usually outside government. Most creative physical scientists are in universities, which is remarkable considering the salary structure. Private industry employs a big proportion of our scientific talent, which means that these scientists are under pressure to serve industrial aims and their loyalties are often diverted from the public interest.

Part of the reason why the scientific community is clustered outside government has been the mismanagement of science by the military. Military power must now be considered primarily in terms of science and technology. Yet military organization and education have not changed to fit the new facts. Obviously the military will need more and more scientifically mature personnel and fewer squadron leaders, but it continues to train squadron leaders. What is more, up to now it has had a negative approach to its selection of scientific management. Processes used to select a good man to run a submarine are applied, despite their inappropriateness, to selecting a man to run a laboratory or to choose between two complex weapons systems. Good scientific managers are automatically weeded out, and poor ones promoted.

Unfortunately, the traditional military organizational structure tends to be inimical to the promotion of scientific progress. It was designed to produce specialists in violence. Now suddenly the most critical task is the selection of highly technical weapons systems—a function for which the military structure is not particularly suited.

But scientists outside government still try to influence matters from the edges by pulling strings and poking their fingers into the wheels. They give generalized advice, but the problems are specific. Someone must choose, for instance, between spending $500 million to make better re-entry vehicles for missiles or spending $500 million to build a completely different missile with a different basing system, and these decisions must be lived with. The kibitzing scientist, not responsible for the consequences of his advice, is at best of limited usefulness; at worst, dangerous.

Responsibility and scientific competence must somehow be brought together if government is to serve the public interest and if the right decisions are to be made to advance the intricate giant that science has become. Having the top ranks of government heavily staffed with people trained in science, who really know how to handle scientific problems, is a solution apparently not available to this country. Obviously it is being tried in Russia.

In the United States the government, lacking scientific expertise, farms out its scientific problems to industry. The ordinary profit-making company has a very limited sense of public responsibility. It may be effective in production and capable of top-notch research and development, but its interests often – and necessarily – diverge from the public interest. There is a tendency to let the government finance the long shots but to seize promising developments and exploit them with company money. Industry naturally tries to exploit governmental support for private gain (within legal limits) and steers the short course of its own health and well-being. If a company is to survive in this quasi-capitalist society, it must look out for itself first. Because of this inevitable self-interest, industry must not be allowed to become the arbiter of national science policy by default.

One promising scheme for handling science and technology in the public interest has been the non-profit organization, or, as they prefer to be called, the public trust organization. The government first used the non-profit device in about 1820, when it gave a contract to the Franklin Institute in Philadelphia to find out what made steam boilers explode. In the last thirty or forty years there has been a proliferation of non-profit organizations which have been extremely effective in basic research, applied research, and even production. A number of these are run by universities, such as the Argonne Laboratories of the University of Chicago, M.I.T.'s Lincoln Laboratory, and the University of California's two weapons research labs and its operation at Los Alamos. There are also private non-profit companies like RAND, System Development Corporation, and Aerospace Corporation.

The main advantage of these organizational inventions is that they are insulated from bureaucratic meddling. They work on governmental problems outside the governmental structure. They typically have a broad charter in which their responsibilities are general, their budgetary restraints non-specific, and monitorship of their operation reasonable. They permit a freer use of scientific talent. They break through the unrealistic ceilings set by government on the salaries of scientists and allow the public service to compete on an even economic footing with private industry. Most important, they are able to maintain an atmosphere congenial to the scientific community.

This kind of freedom is necessary for scientific accomplishment, and the method has proved itself. In terms of technological productivity the non-profit groups have been extremely successful, particularly with the AEC. But the freedom on which their success is based is achieved by a delegation of power from government, and even though they have strong internal commitments to the public interest, and their actions usually serve that interest well, they do not literally represent government.

What is needed is an invention *inside* government equivalent to these non-profit corporations. Within government a delegation of authority and responsibility could be made to large self-contained units. The liberty necessary for a benign environment for science could be preserved, and creative scientists might be lured into government service. Yet the power to direct the course of science and weigh its consequences in terms of the public welfare would not be relinquished. The AEC system, an experiment in governmental management of science and technology, is a significant step in the right direction.

A new and better marriage must be made between governmental responsibility and scientific capability if the full promise of science is to be realized and its perils escaped.

James Real

Almost 80 per cent of all research and development monies are furnished by government, of which all but a small fraction are directed at prompt application to the technologies of warfare and its endless supporting apparatus.

It is unlikely that we shall ever hear again such lines as were delivered in 1958 by a distinguished Nobel Laureate physicist to an assembly of his colleagues. "The scientist," he insisted, "has no idea what disposition will be made of his work. There is usually at least a two-year lag between his discoveries and their unpredictable applications." The Laureate went on to spin out this thesis of disassociation, even though everyone in the hall was intimately aware of the hundreds of laboratories and plants created for and totally supported by government, populated by tens of thousands of physical scientists working cheek to jowl with lesser folk to achieve specific and immediate technological ends.

As incredible as this posture was in 1958, it is now even more absurd. Today there are fourth and fifth generations of scientists who have never worked on anything but weaponry and who view their careers as lifelong. They are permanently dedicated to the invention and construction of what may appear to be a succession of weapons systems stretching through foreseeable time. In a real sense, these men are institutionalized: captive to their narrow specialties and to the paymaster, the grant, and the contract.

The military, who are the ultimate appliers of the laboratory invention, are not threatening to us because of their eagerness to fight or to govern. I believe that they are generally a good deal less belligerent than some of their predecessors in these last twenty-five years. It is the delicate and dangerous gear with which they are charged that raises the specters of the consequences of accident, irresponsibility, or madness, common phenomena of any war, to such heights. And it is the latitude in making decisions for which the military is asking that suggests future perils for us. The military does not object to the decisions once they come; what they complain about is that getting the decisions through the civilian bureaucracy renders the strategic and tactical advantages of modern war equipages useless.

What is the value of computerized, highly mobile war gear, they ask, when the opponent can come back in an hour with a decision that takes us three days to make and transmit? It should be apparent that a major crisis of decision will some day, somewhere, once and for all tumble the system whereby ultrasonic weapons and their attendants are controlled by the ponderous machinery of nineteenth century decision-making processes.

It is clear that weapons diplomacy, the application of force as the trump card in international relations, is archaic. Worse, it is useless. To think otherwise, one is forced to ignore the microsecond weapons systems which have created such an unbearable crisis in international political decision-making processes everywhere, especially in the democratic societies.

My contention is that it does not have to be left this way; that perhaps before it is institutionalized completely, the scientific community can make a massive attempt to balance the war system which they have bestowed on the republic with devices and systems to block its use. They can decide to turn a portion of their interest from the redundancies of thermonuclear overkill and the diversions of outer space to the aid of the political process and the real defense of the free society. Specifically, I am asking if it is not possible to build into the framework of democratic governing processes advanced technological systems that will give us a chance to understand the current conditions and attitudes of the rest of the world, its peoples, and its leaders; devices that will enable us to abort crisis situations or, once they are upon us, provide us with alternatives to violence.

There are obstacles to any significant movement of science toward a concentrated assault on problems of this magnitude. For one thing, they are hard. Science, for all its awesome façade, now likes to do easy things. A large portion of the physical science population has been immersed in polishing inventions twenty or more years old. The behavioral and social sciences, bemused by access to electronic counting gear, each year load the trade magazines with projects of increasing triviality. In spite of some progress, the scientific pecking order is still much as it has been, rigidly segregated by craft status and increasingly insulated, one discipline from another, by staggering inventions of professional syntax.

In a very few areas, attempts are being made to attack the problems of the social and political orders by at least asking questions of the technicians stultified by their long tenure in the weapons business:

What, if anything, can the wondrous machines do to help us assess the hopes, fears, and aspirations of the world in a continuous way? Is there, for example, nothing science can do to close the technical gap between doorbell-ringing opinion-gathering methods and the capacity of the million-bit memory drum, which is now sometimes diverted to such uses as predicting the best bus schedules from California to a Nevada gambling house?

Is there no better way to guide our governors than by the guesswork of the people who have elevated themselves to the role of "operations analysts" and who, for lack of our possession of better methods, profoundly affect the gravest decisions of history?

What, we ask, is "credibility"? Is it the same to one man as it is to another?

In the same patois, what is "rational behavior"? Is it the same to an Israelite as to a Formosan, to a Japanese as to a Nebraskan?

What are the components of "threat" that finally tote up to being "intolerable"?

Can incipient paranoid behavior out of the forces of complex circumstances be predicted in a people or their leaders? If not, a useful understanding of mass behavior is not foreseeable, and most of psychiatry, psychology, and a good deal of physiology must be marked off as limited individual therapeutic techniques.

The questions go on, inferentially urging all the disciplines of science to consolidate and press a fraction of the ingenuity and energy that has gone into the war system toward an information gathering and analysis system that can begin to help us out of the horror that by 1965 will cause the equivalent of thirty-five tons of TNT to be assigned to the personal containment of every human being then living on the globe.

Walter Lippmann has warned that neither the United

States nor Soviet Russia must push the other beyond that point of provocation and humiliation at which even the most rational nation "can be provoked and exasperated to the point of lunacy where its nervous system cannot endure inaction—where only violence can relieve its feelings. It is the business of government to find where that line is—and to stay well back of it." And, I would add, it is the business of science to help government find and hold the line.

Science must mount an unprecedented effort to furnish government with an assessment system that draws on the pertinent knowledge of all its branches and to transmit it in usable form to the managers of the political and the military systems. The scientist no longer has the right to remain apolitical. These efforts will have to be launched, maintained, argued, and defended by individual scientists. For example, since money is not only the lubricant but the propellant of scientific development, the scientist himself must start to influence the disposition of governmental research and development funds.

I am *not* asking for an overlying organization of scientists to tell us what to do and how to do it. I *am* asking for the attention of the individual scientist who is now immersed in weaponry or in the Next Fifty Years at Bell Labs. Science and its common-law wife, technology, have bathed long enough in the adulation of the popular press and in the awe in which great segments of the society have held them because of their creation of such impressive murder machines. Now they must turn to inventions of far greater novelty, complexity, and importance. The mounting of the thermonuclear war machine has stultified international order and crippled our hopes to revive it by traditional political and social means. Now science must somehow furnish us a parallel system of equal impressiveness under which their highly refined system of murder machines may be controlled.

Lynn White, Jr.

About a hundred and thirty years ago Auguste Comte schematized human history in terms of three ages: the age of religion, the age of philosophy, and the age of positive knowledge or science. He had faith in science, and his positivism is the heart of modern orthodoxy. All of us today take for granted that humanity is progressing from bondage to mastery of the natural environment, from superstition to knowledge, from darkness to light. It is axiomatic that science is the exploration of an endless frontier and that its processes cannot be reversed or even seriously interrupted. Every American or European, every Asian or African deeply influenced by Western culture, has implicit trust in the inevitability and rightness of this onward sweep of science. Even the churches embrace the new orthodoxy, if they are judged more by what they do not say than by what they say.

The modern positivist is a man of faith as much as was the medieval mystic. The concept of human destiny secularized by Comte was evolved by Joachim of Flora, a Cistercian abbot of the late twelfth century, who divided history according to the Trinitarian dogma, equating the ages of the Father, the Son, and the Holy Ghost with an age of fear, an age of love, and an age of freedom. Joachim's vision was taken up by the left wing of the Franciscan movement and broadcast over Europe. It was inherent in the thinking of late medieval and early modern proletarian revolutions and underlies the Marxist straight-line notion of human destiny. When Comte transmuted Joachim's formula, he was replacing one faith with another closely related to it.

No faith can afford to reign unexamined. Our habit of regarding scientific progress as inevitable may in fact be dangerous to its continuing vigor. In every civilized society something that can legitimately be called science has existed, but the amount of energy put into it has varied enormously. In every age minds of great ability are attracted to the focus of cultural interest, be it the fine arts, literature, religion, science, or something else. If the cultural climate shifts, the concentration of intellectual energies and capital investments follows.

Science must have a positive emotional context to thrive, as well as economic and political encouragement. Legislatures and corporate bodies must reach decisions favorable to science, and investors and voters must approve what their representatives do. Parents must want science in the education of their children. Above all, a significant proportion of the ablest minds must choose to dedicate themselves with passion to scientific investigation if the movement is to progress.

The modern outburst of scientific activity is not necessarily permanent. The cultural support that science enjoys today rests more on fear of foreign enemies and of disease than upon understanding, and fear may not be a healthy or lasting foundation. Science needs its statesmen, and statesmanship demands the long view. The future of science, like its past, will be largely a matter of accident unless measures to assure its continuance are attentively sought. Since the energy that civilization expends on any activity depends on the cultural climate, the important question today is: What can be done to insure an affirmative social context for science?

The historian has no ready answers. No professional historian thinks that history repeats itself. History does not foretell the future, but study of the past may provide some keys to understanding. Above all, knowledge of history should liberate us from the past and enable us to be vividly contemporary. Viewing human experience in vastly different circumstances helps to dislodge presuppositions, and may free our ideas about what needs to be done to assure the future of science.

The prestige of science today sustains a common but false assumption that any robust culture must have had considerable scientific activity. Now Rome was immensely vigorous. Languages descended from Latin are still spoken from Tijuana to Bucharest. The overwhelming mass of legal structures of the world, not only in Europe but in Asia and the Communist countries as well, is descended from Roman law. The Romans had vast creative ability and originality; yet there was no ancient Roman science. Nothing that can be called science existed in the Latin tongue until the twelfth century. From our modern point of view, Roman indifference to Greek science was absolutely spectacular. It has been argued that by the time of the Roman Empire Greek science was so far past its great days that it could not attract the vigorous Roman mind. But distinguished Greek scientists, such as Galen, lived for long periods in Rome. As for the "petering out" of Hellenic science, one of the most original Greek scientific thinkers, Philoponus of Alexandria, was contemporary with Justinian in the sixth century. Greek science was available to the Romans, but was ignored.

Even more disconcerting is the case of Islamic science. During some four centuries, from roughly 750 to 1150 A.D., Islam held the lead in scientific activity. In the eighth century a government-supported institute of translation emerged in Baghdad. Very nearly the complete corpus of Greek science and a major part of Indian science were made available in Arabic within about eighty years. Original scientific work began appearing in Arabic by the late ninth century, especially in mathematics, optics, astronomy, and medicine.

In the early tenth century Al-Razi, an Islamic physician, produced a book known eventually in Latin as *Liber Continens,* an encyclopedic codification of Greek and Hindu medicine, including a great deal of Al-Razi's own observation. It is probably the biggest single book ever written by a medical man, and is a superb work. In 1279 it was translated into Latin for Charles of Anjou by a Jewish physician of Agrigento in Sicily. It was published in Brescia in 1486 and reprinted four times before 1542. It was a fundamental medical reference book for centuries, and was entirely absorbed into the stream of Western medicine. But perhaps the most striking thing about it is that no complete copy of Al-Razi's great medical encyclopedia exists in Arabic. It was practically forgotten in Islam after a few generations.

The Arabic-speaking civilization knew what science was and was proficient in it. For four hundred years science was one of its major concerns. But a crystallization of other values occurred in the late eleventh century which shifted the whole focus of Islamic culture. Science was abandoned, and abandoned deliberately.

Christianity's relation to scientific activity has varied greatly through the ages. It has been said that early Christianity killed Greek science; but Christians were no more indifferent to science than were contemporary pagan Romans. The early Christian attitude was based on the view that natural phenomena were relatively unimportant. Only spiritual values had significance. The natural world deserved attention solely because God used it to communicate specific messages to the faithful.

This concept of the function and nature of the physical world is illustrated in a sixth century story about Pope Gregory the Great. Gregory, not yet Pope, had seen English slaves in the Roman slave markets, and decided to evangelize this pagan people. He received permission from the then Pope and started for England. On the evening of the second day out, while he was resting and reading, a locust – *locusta* in Latin – hopped up on his book. He knew that God was speaking to him. The Latin words *loco sta* mean "stop"; he took this to be the meaning of the message and went no farther. The next day couriers from Rome reached him and summoned him back. The people of Rome had demanded that the Pope recall Gregory from what would have been a lifelong mission because they desperately needed his leadership.

It is plain that science could not flourish in a culture that held to such a "rebus" interpretation of natural phenomena. But by the twelfth century this attitude began to change, at least in the Latin West. People began to pay more attention to the physical world. Sculpture of the early Gothic period clearly shows that the artist looked at real vegetation when he carved ornamental leaves or flowers. In the thirteenth century St. Francis of Assisi supplemented the doctrine that material things convey messages from God with the new idea that natural phenomena are important in themselves: all things are fellow creatures praising God in their own ways, as men do in theirs. This new notion opened a door to natural science, and partly explains the enthusiasm for experimental science in the Franciscan order at that time.

Another concept crucial for the whole development of modern science was emphasized in the thirteenth century and found its clearest spokesman in the Franciscan friar Roger Bacon. He said that there are two sources of knowledge of the mind of God–the Book of Scripture and the Book of Nature–and that each of these must be searched by the faithful with equal energy. He pointed out further that study of the Book of Nature had been sorely neglected.

This idea, natural theology, changed the role of men from passive recipients of spiritual messages through natural phenomena to active seekers for an understanding of the Divine nature as it is reflected in the pattern of creation. Natural theology was the motivational basis of late medieval and early modern science. Every major scientist from about 1250 to about 1650, four hundred years during which our present scientific movement was taking form, considered himself primarily a theologian: Leibnitz and Newton are notable examples. The importance to science of the religious devotion which these men gave their work cannot be exaggerated.

Why did the idea of an operational natural theology emerge in the thirteenth century, and in the Latin West alone? There was no similar development in Greek Christendom. It may have sprung from the key religious struggle of the time, the battle of Latin Christianity with the great Cathar heresy. Early in the thirteenth century it looked as though the Cathars were going to get control of a strip of territory extending from the middle Balkans across northern Italy and southern France almost to the Atlantic coast, separating the Papacy from the more orthodox areas of northern Europe. The Cathars' major doctrine was that there are two gods–a god of good and a god of evil. The visible universe is the creation of the god of evil, which means that living a good life involves having as little as possible to do with physical actuality. Christianity holds that matter is the creation of the one good Diety. In the process of upholding the Christian position against Catharism, natural theology assumed a new relevance and vividness.

Natural theology was unquestionably a major underpinning of Western science. By the time the theological motivation began to diminish, Western science was formed. Today the motive force of natural theology has long been spent, and it does not seem to have been replaced with any other idea of equal power. Are modern scientists quite sure why they are pursuing science? Science is fun, and the exhilaration of the chase may keep it going for a long while. But will scientific advance continue without more serious impulsion?

Scientists must become increasingly aware of the complexity and intimacy of science's relationships to its total context. The modern tendency to regard science as somehow apart from, or even dominant over, the main human currents that surround it is dangerous to its continuance, and can be harmful even to progress within science. The veneration of the circle is an example of a general presupposition that constricted even so great a scientific mind as Galileo's. Galileo, in bondage to the axiom that the circle is the perfect curved form and therefore necessary to any significant speculation, could not seriously contemplate Kepler's thesis that the planets move in elliptical orbits. He neither accepted nor refuted Kepler's notion. He committed the unforgivable sin: he disregarded it.

Fixation on the circle was almost complete in ancient culture. The Romans recognized only three ovoid forms: in arenas, in shields, and in the bezels of rings. Pagan Scandinavians used the oval for a type of brooch, but discarded it as soon as they were Christianized, i.e., Mediterraneanized. The Middle Ages had no oval forms except occasionally the nimbus surrounding Christ in scenes of the Last Judgment or the Ascension, and even this was a version of the ancient Christian fish symbol, pointed at both ends. As late as the fifteenth century, artists could not draw a picture of the Coliseum which showed it oval. The first ascertainable oval design in a major European work of art is the paving that Michelangelo designed in 1535 for the remodeling of the Capitoline Piazza in Rome. Michelangelo and his successors during the next fifty years created an atmosphere in which ovoid forms became respectable, until finally Baroque art was dominated by the oval. Kepler's astronomical breakthrough was prepared by the artists who softened up the circle and made variations of the circular form not only artistically but also intellectually acceptable.

While the sanctity of the circle long impeded science by closing avenues of speculation, another inherited classical idea of a very different sort restrained progress by divorcing thought from practice. Manual labor was extolled for seven hundred years by monks, especially the Benedictines, as being not merely expedient but spiritually valuable as well. With the late medieval revival of Greek and Roman attitudes, however, the classical contempt for manual labor reasserted itself. The universities emerging in the thirteenth century had faculties in the liberal arts, law, theology, and medicine. Medicine was the only discipline with an embarrassing manual aspect, and in order to retain their prestige the medics separated surgery from medicine. Surgeons did not want to be downgraded either, so surgery became largely theory. There are pictures showing a professor of medicine lecturing to students, while a theoretical surgeon in turn directs a barber surgeon who dissects the cadaver. Medicine advanced during the latter Middle Ages, but it seems likely that it advanced less rapidly than would have been the case if the study of surgery, anatomy, and medicine had been carried on by the same people. Speculation too far removed from substance is often of limited value. The trend to purge university curricula of "vocational" courses may contain a seed of decay.

Current discussion of the problems of maintaining scientific progress usually focuses on the importance of providing an adequate economic base for science and creating an atmosphere of political and intellectual freedom in which science may flourish. But, as we have seen, changes in science in the past have also to be related to changes in basic religious attitudes, in aesthetic perceptions, and in social relationships. More of our attention should be directed to an examination of the sources of our faith in science today, and to the wellsprings of motivation that lead men to pursue science. Why does a man become a scientist? Why does he choose his manner of work, and how does he select the area that engrosses him? The answers to questions like these are not entirely economic or political.

Our science itself may contain unexamined axioms, like the circular prison that held Galileo captive. Hypnotism is an example of a phenomenon that science has not really tried to explicate, apparently because in some way it seems outside accepted categories of "reality," although it has been used in amazing ways in dentistry and surgery.

A distinguished surgeon told me about a delicate heart operation carried out under hypnotism, and added, "That sure is fooling them." But who is being fooled?

The continuation of civilization as we know it depends on science, and the continuance of science would seem to depend on our ability to examine this sphere of human activity objectively and relate it to its human context. Those responsible for the statesmanship of science must develop a scientific understanding of science itself. They must become increasingly aware of the intricacy of the ecology of the scientist. We must learn to think about science in new ways unless we intend to leave the future of science to chance.

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The Fifth Estate in the Seventh Decade

The status of science and scientists in the 1960's is reviewed.

Paul M. Gross

My distinguished predecessors of the past few years who spoke on similar occasions as retiring president of the AAAS dealt with various substantive aspects of science. At Chicago in 1959 Paul Klopsteg, in a talk reminiscent of Bragg's famous essay on the contribution of British craftsmen to British science, depicted the role played by instrumentation in the development of science. The next year, in New York, Chauncey Leake described the development of a special area of science—that of pharmacology and physiology. Last year in Philadelphia Tom Park gave us an account of the origins, development, and outcome of his own research program as an investigator in biology.

Tonight, instead of talking of my own field of physical chemistry, I think it may be of interest if I say something of the present status of science and of scientists, as I see it, from my experience of almost a half century in scientific education, research, and administration. The reference in the title to the "seventh decade" is, of course, obvious. That to the "fifth estate" may not be familiar to some. This relates to the three estates of English history—the Lords Spiritual, the Lords Temporal, and the Commons. To these was added a fourth—by Edmund Burke, according to Carlyle. Burke is said to have observed, in a famous speech: "There were Three Estates in Parliament; but, in the Reporters' Gallery yonder, there sat a *Fourth Estate* more important far than they all." If he were speaking today I am sure he would enlarge the gallery considerably and provide ample space for the commentators and columnists who, obviously, know all about the world and its affairs, both scientific and otherwise. So much for the fourth estate.

The "fifth estate" of my title can best be described in the words of the distinguished scientist and technologist Arthur D. Little, who first used this term in an address in 1924 at the centenary celebration of the founding of the Franklin Institute.

> This fifth estate is composed of those having the simplicity to wonder, the ability to question, the power to generalize and the capacity to apply. It is, in short, the company of thinkers, workers, expounders and practitioners upon which the world is absolutely dependent for the preservation and advancement of that organized knowledge which we call "science."

The status of science and scientists in the 1960's is obviously a large subject, and here I will discuss four aspects of it which I feel should claim our attention, thought, and understanding. The first relates to the greatly expanded tempo, scope, and power evident in the development of science during the past quarter of a century. Secondly, I would like to consider the increasing role of science and technology as an instrument of national policy. A third area meriting attention is a changing pattern of scientific activities and some implications of this. Lastly, and most important, for the future advancement of science, is the place of science and scientists in our modern social structure and the interactions with that structure.

Changes in Tempo, Scope, and Power

Before elaborating on these topics, I think it desirable, even at the risk of covering ground familiar to many, to sketch briefly against their historical background some of the scientific developments familiar to us. In doing this I will attempt to emphasize not the content of science so much as the changing characteristics of scientific endeavor. For this purpose it will be convenient to have two reference points in time: the late 1800's just prior to the turn of the century and the decade from 1925 to 1935.

While the ranks of the fifth estate have grown rapidly in this century, it is still true that the number of scientists remains a small fraction of the total population. In the long years prior to 1900 the voice of science in national and world affairs was rarely heard, and the individual scientist working in his ivory-tower laboratory was a little-known member of society. Nevertheless, contributions of science and technology to human welfare and to the problems of the military, the growth of industry, and economic development in general were more important with each passing decade. Toward the end of the last century and in the early years of this one a new aspect of scientific activities began to emerge. This was the concept of highly organized team activity in scientific and industrial research. It was in Germany that this concept first appeared in any substantial measure, in the latter years of the 19th

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century. Its effective utilization gave Germany a leading position in producing such things as chemicals, pharmaceuticals, steel and machinery, and similar products of industries where scientific and technical knowledge was a prerequisite for effective production. This lead over other countries, including the United States, was retained up to World War I. Some of us can recall hearing the news in the early years of that war, before our entry, that the German submarine *Deutschland* had successfully eluded the British naval blockade and landed in Baltimore harbor. What may not be as well known is the fact that the cargo consisted of scarce pharmaceuticals and dyestuffs which sold at high prices in this country because of our almost total dependence on Germany for such synthetic chemicals. On the return voyage the cargo was mainly tungstic oxide, as tungsten was a critical raw material in many areas of Germany's advancing technology. Only after World War I and as late as the 1920's did the industrial research concept of today begin to appear as an important component of some of our own more technically based industries.

The world nitrogen supply and the fate of nations. Before this century much scientific thinking was still limited in its scope and heavily circumscribed by the walls of the laboratory. There were of course exceptions. Though the ranks of science were small in number, they included a goodly share of giants—such men as Maxwell, Rayleigh, Herz, and Röntgen, to mention but a few. One in the field of chemistry was Sir William Crookes, president of the British Association for the Advancement of Science at its Bristol meeting in September 1898. In his presidential address, after an excellent analysis of factors bearing on world food supplies, he spoke as follows.

> The fixation of nitrogen is vital to the progress of civilized humanity. Other discoveries minister to our increased intellectual comfort, luxury, or else convenience; they serve to make life easier, to hasten the acquisition of wealth, or to save time, health or worry. The fixation of nitrogen is a question of the not too distant future. Unless we can class it among the certainties to come, the great Caucasian race will cease to be foremost in the world, and will be squeezed out of existence by the races to whom wheaten bread is not the staff of life.

That these are still matters of vital interest today is seen by recalling current discussions of the population explosion, and discussions of this past fall relating to the sale of surplus wheat from this and other countries to help feed the millions in the Soviet bloc.

While we all have general awareness of the important role of organized industrial research in defense and in economic development, this can be focused more sharply by looking back at the events relating to the world's nitrogen supply that occurred after 1898. Based on fundamental research in Germany and Scandinavia, in the period between 1900 and World War I a new industry developed, that of nitrogen fixation. However, this was of only limited capacity at the beginning of the war. Since nitrogen is essential not only for agriculture but also for the manufacture of explosives, as war became imminent Germany began stockpiling Chilean nitrate. The first important naval engagement of World War I was fought not in the Atlantic but in the Pacific, off the coast of South America, in an operation in which British warships captured or sank a German merchant convoy carrying Chilean sodium nitrate back to Germany. With this event there were many predictions that Germany could not last long in the war, with her very limited domestic sources of nitrogen. As with many predictions, these proved quite wrong in the outcome. During the war years, the Germans, with their by then matured capability in industrial research, were able to build the first major nitrogen fixation industry in the world. Moreover, after her recovery from war and with the rebuilding of her commerce, Germany became the world's principal producer of nitrates and supplied these to Europe and the Atlantic seaboard at prices with which Chileans could not compete. The next step in this chain of economic events was a fiscal crisis in Chilean affairs, as a substantial part of the Chilean economy had been based for years on a tax on exported nitrates. Recovery from this crisis came only when, through research sponsored by American financial interests, more efficient ways were found of mining the Chilean nitre deposits and of extracting and marketing as a valuable by-product the significant amounts of iodine that they continued.

This illustration of changing conditions in the nitrate industry is but one of many that could be cited. In today's highly technological civilization the fate of nations will depend increasingly on their store of scientific knowledge obtained through basic research, and on their capacity and ingenuity in applying this knowledge to produce goods and provide services of all kinds. This is the basis of a sound economy and the key to its forward progress.

Rather than continuing with an account of more recent scientific and technologic events familiar to all, I will simply point out that greatly expanded basic and applied research between World Wars I and II and after World War II led to such results as the high state of development of the airplane for transportation, the whole electronics industry, the release of nuclear energy and its use for power and the propulsion of naval vessels, and, finally, the successful launching of orbiting satellites.

International Geophysical Year. More detailed review of similar developments would quickly reveal much to support the thesis that there has been a greatly expanded tempo, scope, and power in activities in science during the past quarter of a century. So far as scientists themselves are concerned, this could almost be regarded as the emergence of a kind of fourth dimension in scientific thinking. Justification for such a statement is evident in a number of directions—for example, in the thinking, planning, and execution that went into the project known as the International Geophysical Year. This was a bold frontal attack, involving international collaboration on a grand scale, which was made in an attempt to understand more fully the physical nature of the surface of our globe through a carefully planned survey of the scientific phenomena relating to the atmosphere, the oceans, and the input of radiation of all types to our near geosphere. The information gathered was vast, and the discoveries were many. Their significance for a better understanding of such important phenomena as weather changes and climatic cycles is already apparent. As the many scientists interested in this area continue working on the large number of data that were accumulated, a much deeper knowledge of the surface of our earth can be expected.

Another example of the type of thinking I have referred to, and one still on the scale of great dimensions, is the project currently under way which is known, for short, as "the Mohole." This is an attempt, fraught with great difficulty, to penetrate the earth's crustal layers to acquire a better understanding of the nature, composition, and behavior of its massive interior core. However, such scientific thinking and progress have not been confined to endeavors of large dimensions, even global in scale. In the past decade, work of a highly competent team of mathematicians, physicists, chemists, and biochemists at Cambridge University has led to a better comprehension of the basis of life processes, through discoveries of great significance in the field of molecular biology. The determination and unraveling of the complex molecular structure of giant molecules, such as ribonucleic acid (RNA) and deoxyribonucleic acid (DNA), have been major advances and outstanding illustrations of the effective collaborative, scientific teamwork so characteristic of much current scientific activity.

Possible modification of the climatic cycle. A final example of thinking of this sort is a proposal by Ewing and others for possible modification of the age-old climatic cycle which results in repetitive glaciation of continental land masses south of the Arctic Circle. The geologic and related evidence from prehistoric times for the existence of such a cycle of ice ages with a period of perhaps 30,000 years appears clear. Ewing's thesis, in broad terms, is that the occurrence of this cycle is related to the extent of accumulation of ice and snow on the polar ice cap within the Arctic Circle and also the ingress and exit of warmer waters from the Pacific and the Atlantic over the edges of the fairly shallow geologic basin which holds the Arctic Ocean. The conclusion of the argument, which I shall not develop fully, is that this cycle could be altered by stopping or at least modifying the flow of water through Bering Strait between the Arctic Ocean and the Gulf of Alaska in the North Pacific. This would indeed be a gargantuan project in applied science, execution of which could only have been thought possible—whether desirable is another question—with the availability of nuclear explosives. Thinking of this type would, in my judgment, have occurred but rarely in earlier periods of the development of science.

An Instrument of National Policy

Much that I have outlined is evidence of the increasing role of science and technology as an instrument of national policy—the second topic under discussion. An illustration from behind the Iron Curtain at once comes to mind.

Few of use like the tenets of Soviet ideology, though many take complacent comfort in the disparity between the present standard of living in Russia and our own. Nevertheless, Russia has forged ahead through the encouragement of science, through the systematic employment of the methodology of research and development, and through the extension to large segments of her population of free education oriented strongly toward rigorous training in science and technology. Nicholas DeWitt, who has studied the Soviet manpower and educational system intensively, states the situation in the postscript to his book *Education and Professional Employment in the U.S.S.R.*

> If the aim of education is to develop a creative intellect critical of society and its values, then Soviet higher education is an obvious failure. If its aim is to develop applied professional skills enabling the indivdual to perform specialized, functional tasks, the Soviet higher education is unquestionably a success, posing not only a temporary challenge, but a major threat in the long-run struggle between democracy and totalitarianism.

While DeWitt's first description of the aim of education may well give us pause when we think of values in relation to our own culture and society, and make us ask how well our own system of education has done, the validity of his concluding statement becomes apparent from the perspective of little more than a third of a century. In this short period Russia rose from the rank of a third-rate power to a position, today, second only to that of our own country.

National defense. There is a final element relating to the present role of science and technology as an instrument of national policy which must be mentioned. This concerns warfare and the preparation for warfare, or what is today euphemistically called national defense. From the first development of gunfire in the 14th century there had been little real innovation in the practice of warfare until this century, though the Civil War did bring the introduction of steel armor and the submarine. In World War I, gas warfare, tanks, and the aeroplane made their appearance. The development of the latter for military use between the two wars paced and enhanced the great development of commercial aviation, and this, in turn, has reduced passenger traffic on our widespread network of railroads to a fraction of its volume in the first third of the century. In World War II, radically new concepts such as the proximity fuse, the landing craft, and, of course, nuclear explosives were introduced. The war also saw the refurbishment and effective use of a very old device—the rocket. This was first used as a weapon by the Mongols about the middle of the 12th century, and it reappears from time to time in the subsequent history of warfare in various military versions. Francis Scott Key witnessed one of these occasions when he was a prisoner in a ship in the British fleet off Baltimore at the siege of Fort McHenry in 1814, and the spectacle inspired the line in our national anthem: "And the rocket's red glare, the bombs bursting in air. . . ."

The high state of effectiveness to which rockets were brought toward the end of World War II through intensive research and development and the advent of the German V-I's and V-II's provided the background for today's missile technology. Further development of long-range offensive missiles provided the launch rockets for orbiting satellites and for vehicles for space exploration. These are some of the advances that have completely changed the whole aspect of warfare in less than a third of a century. In this area there can be no doubt that scientific advance and capability are indispensable instruments of national policy.

Changing Pattern of Scientific Activities

What, then, have been the effects of this great expansion of science and technology, this changed scientific thinking, this involvement with national policy, on science itself, on its organizational patterns, and on scientists and their pursuit of scientific endeavor? The question brings me to my third topic. These effects have been both major in scope and diverse in direction. They have been both favorable and unfavorable for the sound advancement of scientific endeavor.

Consider first the positive side of the coin. Today, the nature and tempo of effective research requires ample funding for men, machines, and facilities, and funds have been made available in rapidly increasing measure during the past third of a century. A glance at one area, that of nuclear and high-energy physics, will quickly reveal the scale and pattern of support. From relatively small beginnings, in such laboratories as those of Rutherford and the Curies, nuclear physics in the 1920's and 1930's moved steadily but slowly ahead. Support for the first generation of high energy machines, the early cyclotrons, came mainly from university funding and private giving by individuals or foundations. The demonstration, in the early years of World War II, of the feasibility of the nuclear chain reaction and of its significance for the release and utilization of nuclear energy in war and peace led quickly to federal support, first through the Manhattan District Project and later through the Atomic Energy Commission. The scale of this support was not in millions but in billions, and this pace continues today. However, the magnitude of expenditures, though indicative of the scale of modern scientific activity, is never a good measure of scientific achievement.

Astronomy. Nevertheless, a brief survey of several areas of science will reveal that great substantive progress has been made in recent years. A case in point is the field of astronomy. Next to mathematics, this is the oldest of the sciences, dating from Babylonian times in the 3rd century B.C., and it has a long history of achievement before 1900. The early years of this century saw the establishment, largely through private philanthropy, of a few observatories, such as that on Mt. Wilson, with telescopes and ancillary instrumentation larger and more effective, by an order of magnitude, than anything that had gone before. These were the forerunners of the large-scale scientific facilities familiar today—the giant cyclotrons, accelerators, and piles of nuclear physics. As astronomy moved ahead through the first half of the century, its progress was relatively slow by comparison with the burgeoning development of the laboratory sciences of chemistry and physics, which received much of their steadily increasing support from private, industrial, and government sources.

It was only after the establishment, at mid-century, of the National Science Foundation to support basic research that attention was turned to more adequate support for astronomy. In the middle 1950's the establishment by NSF of the Greenbank Observatory as a National Radio Astronomy Observatory marked a turning point in the character of federal support for basic science and fundamental research. From this beginning, federal funds became available for "national" basic scientific enterprises, such as the International Geophysical Year in 1957–1958 and, later, the Kitt Peak National Observatory in Arizona (near Tucson), the Mohole project, and the Atmospheric Sciences Center in Colorado. With this type of support, scientific progress in a number of relatively neglected fields, such as astronomy and various branches of the earth sciences, notably oceanography, was greatly accelerated, and the development of pure science for its own sake became, and now is, an acknowledged instrument of U.S. national policy.

The individual scientist and organized endeavor. It is of interest to consider the effects of these changes on scientists themselves. These are manifest in a number of directions, but here I mention only two, which appear to be the most significant. The first may be described as a type of dilemma with which the individual scientist appears to be increasingly confronted. In earlier periods the role of the individual scientist stood out clearly, and while the magnitude of his contribution might occasionally be large, usually it was small, though still real and discernible. Each small contribution was a piece in a growing mosaic of knowledge of the particular field involved. As this mosaic grew from initially few pieces of data and information, and as the basis for their interpretation and correlation became dimly recognized, there was ample scope for individual initiative, and there was wide freedom of choice and of action. As progress in the field increased, the few individuals with greater insight helped shape the pattern of the whole and made it part of scientific knowledge. Much of this was a seemingly random and quite haphazard process.

To this somewhat inadequate description of science in earlier years should be added the description given by Langley in his presidential address before the AAAS meeting in Cleveland in 1888. He characterized the pursuit of scientific research as "not wholly unlike a pack of hounds, which, in the long-run perhaps catches its game, but where, nevertheless, when at fault, each individual goes his own way, by scent, not by sight, some running back and some forward; where the louder-voiced bring many to follow them, nearly as often in a wrong path as in a right one; where the entire pack even has been known to move off bodily on a false scent. . . ."

Whether or not either of these descriptions is an adequate picture of earlier scientific endeavor, it is clear that, in spite of limitations of support, facilities, and equipment, there was ample room for individual freedom of choice and for the exercise of initiative, ingenuity, and resourcefulness. Out of this situation developed what we all inherit and cherish as the great tradition of freedom in science and of communication in science, both nationally and internationally. This may be stated otherwise by saying that science is universal and knows no bounds of geography, race, creed, or nationality. Many attributes characteristic of scientific endeavor in earlier periods still hold for the sharply quickened and greatly expanded domain of today's science. Unfortunately, there are signs that as this domain grows further, as it becomes more highly organized, more programmed, and more directed toward national and other ends, and as its impact on our culture and society becomes more widespread, some of this traditional freedom will be lost. An obvious example of this trend relates to freedom of exchange of information, so essential to the progress of science. In World War II it was found necessary to impose a cloak of secrecy and classification on research in the developing field of nuclear physics—research which led to the release and utilization of nuclear energy. All agreed that this secrecy was necessary in wartime, and it was imposed under the Manhattan District Project. In the early days of the activities of the Atomic Energy Commission these restrictions were still dominant, and it was only with the passage in 1954 of the "Atoms for Peace" modification of the original Atomic Energy Act that some of them were removed or considerably relaxed.

This is one aspect of the so-called dilemma that many see ahead as the role of science becomes more important in modern civilization. Another, perhaps more important but more subtle, aspect can best be illustrated by an example from the field of chemistry. One of the great discoveries by Rayleigh and Ramsey at the end of the last century was that of the existence of the family of rare gases, the description of their properties, and the characterization of their chemical behavior. As these gases were studied further by many investigators, it became a tenet of chemical thinking that they were unreactive and would not combine with the other elements and compounds. So strong was this belief that, as theoretical knowledge of chemical reaction and chemical binding developed through this century, an essential element of each new theory of chemical bonding was that it should account for the supposed fact that these gases would not combine chemically with anything else. The first crack in this inviolate image came from the work of Bartlett, who demonstrated the combination of the rare gas xenon to form one of the components in a coordination compound of complex structure surrounding a central platinum atom. As often happens in science, the initial breakthrough was followed closely by others. Soon after Bartlett's discovery became known, further research and experimentation quickly destroyed this image that had dominated thinking in chemistry for some two-thirds of a century. The experiments leading to this final event need not be described in detail, but the circumstances under which they were undertaken are relevant. While I cannot claim to know these circumstances at first hand, the account as it reached me, and as it is given here, is from a source I believe to be authoritative.

Under the system in AEC national laboratories which provides for research participation by scientists from outside the laboratory staff, a young physicist from a small college came to carry on research for a time at the Argonne National Laboratory. In discussing his proposed program with those responsible for general supervision of the Laboratory, he said he would like to attempt to react xenon and fluorine at an elevated temperature. Since most physical scientists were convinced that the rare gases were unreactive, and since this reaction had already been tried in the Argonne Laboratory at ordinary temperatures, it is reasonable to assume that in the discussion that ensued doubts were raised about the wisdom of devoting the investigator's time and the resources of the Laboratory to the attempt. If such doubts were raised, at least they did not prevail, and it was agreed that the young physicist should go ahead with the attempt. The result was a spectacular, unanticipated discovery in the field of chemistry.

When a mixture of xenon and fluorine was heated in a nickel container to 400°C and then cooled rapidly to room temperature, a deposit of white, colorless crystals of the compound xenon tetrafluoride was found, and the long-standing belief that rare gases are inert was shown to be a myth. These events came to their culmination in the summer of 1962. On learning of this discovery, many investigators at the Argonne Laboratory and elsewhere went quickly to work and made other compounds of xenon and fluorine, as well as of certain of the other rare gases. As of the end of 1963, there is already an extensive literature relating to such compounds. In passing, and somewhat out of context, I might note that *Science*, in its "Reports" section [**138**, 136 (1962)] carried the first general news of this important discovery through a communication from the Argonne group dated 2 October 1962. Incidentally, the interval of 10 days from 2 October to 12 October, the date of the issue in which the report appeared (which carried a striking picture of the crystals of xenon tetrafluoride on the cover), probably constitutes an all-time record in the rapid communication of new scientific information through the printed word.

More in the context of the present discussion of the environment in which today's scientists work was a very timely and thoughtful editorial in the same issue (p. 75) by the editor of *Science*, Philip Abelson, entitled "The need for skepticism." The last paragraph of this is well worth quoting.

> There is a sobering lesson here, as well as an exciting prospect. For perhaps 15 years, at least a million scientists all over the world have been blind to a potential opportunity to make this important discovery. All that was required to overthrow a respectable and entrenched dogma was a few hours of effort and a germ of skepticism. Our intuition tells us that this is just one of countless opportunities in all areas of inquiry. The imaginative and original mind need not be overawed by the imposing body of present knowledge or by the complex and costly paraphernalia which today surround much of scientific activity. The great shortage in science now is not opportunity, manpower, money, or laboratory space. What is really needed is more of that healthy skepticism which generates the key idea—the liberating concept.

Of serious concern under present conditions of highly organized and programmed scientific endeavor is whether the freedom, initiative, and originality of the individual will still be able to emerge to play their important roles, so evident in the history of science in earlier periods. It is disquieting to speculate on what the ultimate outcome would have been, in the case cited, if it had been decided not to make the experiment. How long would it have been before the proper conjunction of circumstances occurred again—the individual with faith in his idea and skepticism of established dogma; a laboratory with chemists experienced in handling potentially dangerous fluorine reactions; and last but not least, a supervisory group willing to authorize the trial? Here the conjunction of events was propitious, and the outcome was a brilliant success. Unfortunately, or perhaps fortunately for scientific morale, as science progresses the number of instances in which the circumstances are not propitious is unknown. We can only hope it is small.

Specialization. A subject of much current interest is the rapidly increasing degree of specialization in science, which has paralleled science's growth and expansion in the past 35 years. Consideration of this is important, because of its implications for sound scientific education and also because of the common reaction of the lay public to highly specialized activity of any sort. Specialization in the most general sense is not new. However, when we consider the complexity of our own social structure—the profusion of implements, machines, instruments, and devices—and its specialisms of all kinds, we tend to think the latter are characteristic of, and even in a measure unique in, our society and time. A moment's reflection will indicate that such is not the case. The thoughtful citizen of the great ancient metropolis of Rome, with a population of nearly 2 million persons in the 2nd century A.D., must have been confronted with something of the same situation. The highly organized civilization of the Roman Empire must have required a high degree of specialization on the part of its citizens to provide its food supply, build its aqueducts and public works, and maintain its roads and the government of its far-flung provinces and colonies—not to mention the high state of development of literature and the fine arts. It seems clear that elaborate specialization, comparable in scope to our own, has been a characteristic of all great civilizations, especially those which were highly urbanized.

Nevertheless, it is desirable to consider briefly the nature of specialization itself, particularly that in the realm of intellectual endeavor. Here the intense concentration of an individual on a limited area of special knowledge and his attainment of expertness in his field tend to break the broad pattern of uniformity of the social structure. This is especially true in a democracy. The resulting separation of the individual from the stream of the common affairs of man tends to make the average citizen uneasy. Shaw put this feeling succinctly, in *The Doctor's Dilemma*, when he said, "All professions are conspiracies against the laity."

Much of the extensive specialization in the sciences has some features that can be most clearly delineated by the following comparison: "A salesman is one who begins by knowing a little about everything and who goes on learning less and less about more and more until he ends up knowing practically nothing about everything." On the other hand, "A specialist is one who starts off knowing a great deal about very little and goes on learning more and more about less and less until he ends up knowing practically everything about nothing."

For our present purpose the description of the salesman can be ignored. That of the specialist will bear further scrutiny. The difficulty lies in the common lay conception that what is small or restricted in scope and dimensions is simple, and in its limits amounts to "nothing." Here is one clue, and a very significant one, not only to the common negative reaction to specialization in general but to the general public's understanding of specialization in science.

Scientists, unlike the lay public, have the privilege of appreciating the accomplishments of a truly great specialist as he reveals fascinating glimpses of things to come, when, from time to time, there is a breach in the ramparts that bar us from comprehension of nature. These ramparts are long and formidably complex, as Vannevar Bush implied in his description of science as "the endless frontier." Rarely do they succumb to attack along a broad front; when they do, it is only through the work of a genius—and geniuses are rare in the human race. If science is to move forward, it will be increasingly important that the general public acquire a better understanding and some appreciation of the true nature of scientific specialization.

As we look ahead to yet unconquered areas, we may confidently predict that soundly conceived specialization in science will continue to survive and multiply. Historically, much of the effort in science has related to inanimate things, or to relatively simpler organisms or functions. As our growing knowledge permits us to move more firmly to studies of human behavior and of its psychological, biochemical, physiological, genetic, and other bases, it is possible to envisage new coalitions between psychologists, neuroanatomists, and neurophysiologists; as they grow, these coalitions may develop as specialties, as is the case for present-day biochemistry and biophysics.

With this prospect confronting us, we shall have to consider the negative aspects of the further growth of specialization and to constantly appraise its soundness. This will be especially desirable in developing sound principles to be followed in future education in the sciences. Here the danger is that the form may be mistaken for the substance. To illustrate the problem and not invoke invidious comparison, let us imagine some future specialty that we call neurobehaviorism, for want of a better designation. On what will the validity and worth of such a field, both as a contributor to our knowledge and as a field of endeavor, depend? First, it will depend on how well those in the field are versed in fundamentals of the relevant derivative sciences, such as neuroanatomy, neurophysiology, and neurobiochemistry. Beyond this, and of great importance, it will depend on how well they understand, or can acquire understanding of, principles from the underlying basic disciplines of psychology, mathematics, physics, chemistry, biology, and physiology that are relevant and applicable to the field in question.

By this criterion, the validity and worth of an area of specialization would depend on the firmness and clearness of the pathways from the outer branch to the deep, sound roots of available scientific knowledge. Perry relates an episode that occurred at Harvard in the 1830's, about Ralph Waldo Emerson and Henry Thoreau, which has point in the present context. The then-young naturalist, who was an intimate of the Emerson household, sat quietly in a corner one day while Emerson expounded to English visitors on education at Harvard, saying, "At Harvard College they teach all branches of learning." At this point Thoreau, to the embarrassment of his patron, blurted out, "Yes, but none of the roots." Without vital and continuing sustenance from strong roots, the branches of specialism will bear meager fruit.

Science in Our Modern Social Structure

Let us now turn to the fourth topic of this discussion of the fifth estate in the 1960's—the impacts of these changes in science on our current culture and the response and reaction of the latter to the change. I have already mentioned many of these changes and need not review them, but two additional ones deserve attention. However, before considering these let us look at a few figures for the sake of perspective.

Scientists and technologists have been, and still are, a relatively small minority group in our total population. In 1900 they numbered perhaps 90,000, representing little more than 0.1 percent of a population of about 76 million. Federal expenditures for science in 1900, similar to the federal expenditures for research and development of today, were about $10 million, or between 0.5 and 1 percent of the annual federal budget. The corresponding rough figures for 1963 are, 2.7 million scientists in a population of 190 million and federal R&D expenditures of $14 billion, which now require about 15 percent of an annual federal budget of the order of $95 billion. Thus, scientists, though their number has increased 30-fold since 1900, still comprise a relatively small part, about 1.4 percent, of the total population.

Effects of the drain on federal resources. It is important, first, to consider some of the consequences of this increasing drain on federal resources that is caused by the recent, almost exponential growth of science and technology. Since federal revenues grow at a much slower rate than the economy does as the economy advances, it is obvious that some adjustment in the growth rate of federal expenditures for science must take place. Indeed, this is already occurring, as is evident to anyone who has followed recent hearings before Congress relating to the expenditures for science projected for the next annual federal budget.

One aspect of this adjustment poses a new and serious type of problem that scientists have not faced previously in any substantial measure. With limitation necessary, on what principles is the assignment of priorities to projects in the various fields of science to be made? What are the relative merits, both in a scientific sense and from the standpoint of the national interest, of a new, large accelerator for nuclear physics, costing perhaps $100 million for its initial construction and about a third of that amount for its annual operation; of the Mohole project for drilling through the earth's crust, variously estimated to cost between $50 million and $100 million; of the expenditure of similar sums annually for biomedical research on cancer or the diseases of the heart; and of landing a man on the moon by 1970, at an estimated cost of over $5 billion? There are no clear guidelines on which to base such priority decisions, and their formulation will require a higher order of statesmanship among scientists and those in the upper echelons of government than has existed heretofore.

Effects on health. The rapid growth of science has had a second type of impact on society in our greatly expanded technological and industrial civilization. This growth has been so great that it has already begun to alter man's traditional natural environment. The emerging problems involve such things as air and water pollution, radiation hazards, occupational hazards, and contamination of milk and food supplies, which are now classed under the general head of environmental health problems.

Several years ago I was asked by the Surgeon General to head a committee of 24 members from widely diverse scientific disciplines. The group was to analyze and survey the problems in the environmental health area and to make a 10-year projection of the nation's needs for scientific research relating to environmental health and of its needs for trained manpower to deal with the problems.

These problems are varied, complex, and serious. They range from the provision of adequate sewage disposal for large and growing metropolitan districts to the recently noted higher level of radioactive contamination of caribou meat, which is an important part of the diet of Eskimos in northern Alaska.

The origin of the high levels of radioactivity in caribou meat was relatively simple to trace and understand, though not necessarily easy to control. Certain lichens on which caribou feed were found to absorb relatively larger amounts than most plants of the radioactive trace elements which the soil had received from the debris of fallout.

A simple illustration relating to the matter of sewage disposal in large metropolitan areas will show the complexity of many of the problems involved in environmental health. A number of years ago, in order to handle its sewage disposal without contaminating Lake Michigan, Chicago built a drainage canal in which water from the lake ran across country to empty into the Mississippi. The sewage effluent from Chicago was fed to this artificial running stream. The dilution of the effluent by water from Lake Michigan reduced its concentration to the point where the organic sewage could be oxidized effectively by the dissolved oxygen in the canal waters, and well-purified water was delivered to the Mississippi. As the city expanded industrially, steam plants were built along the canal, and these discharged warm water from their condensers into the stream. Ultimately, the effect of these additions of warmer water was sufficient to raise the average temperature of the canal water by some few degrees throughout the year. With this development the phenomenon technically known as "heat pollution" became operative. Simply stated, the higher temperature reduced the concentration of oxygen in the water, and therefore the capacity of the flowing stream to oxidize the organic matter present. As of several years ago this "heat pollution" had reached such proportions that its effect on the sewage disposal problem of the Chicago area was estimated to be equivalent to the effect of adding a million people to that metropolitan area.

These are but a few examples of the many effects on the economy, on health, and on various aspects of our culture and society of the greatly increased endeavors of scientists. What has been the reaction to these great changes occurring in little more than a quarter of a century? Here two things are relevant—the status of the general public's knowledge of science and its methods and, even more important, the image in the public mind of the whole modern scientific enterprise. A realistic appraisal of these two factors does not give much ground for thinking that the public has a sound comprehension of science.

The nonscientist's view of science. For the great majority of our people, formal education terminates with high school. Sober reflection about our educational system, after the launching of Sputnik, clearly revealed the woeful inadequacy of the science education of most of our people as a basis on which to build any real understanding of modern science. Since Sputnik, real improvement has been made in science teaching in many of our lower schools, but the effects of this in the adult population will not be evident for another generation.

Given this lack of any sound comprehension of science, what picture can be drawn of the image of science in the public mind? This image is difficult to describe, for it is compounded of many diverse elements. These include respect and gratitude for the "miracles" of modern medicine; admiration for the know-how of applied science which can put satellites in predetermined orbits; and awe, verging on fear, of the results of the mysterious release of nuclear energy. Two events in our time must have contributed greatly to the building of such an image, since the public, as well as most scientists, had no warning and little preparation for their advent. The first was President Truman's unheralded announcement of the dropping of the atomic bomb—a spectacular but terrible demonstration of the power of modern science. The second was the sudden news, one day in October 1957, of a second satellite orbiting our planet. This was Sputnik, the first of a growing family which later included Echo I—a "star" whose rapid course across the night sky could be easily followed with the naked eye.

What is the significance of this image, and of this lack of real understanding by the general public, for the future of science and scientists? Some already feel that scientific endeavor must be controlled and circumscribed if it results in pollution of air and water, in contamination of food with pesticide residues, in the hazards of radiation and the development of nuclear weapons. Still others, left ever farther behind in their understanding of rapid scientific advance, take refuge in a polite, but neutral, type of anti-intellectualism toward all scientific activity. The emergence of attitudes like this among nonscientists is the basis of Snow's discussion of the Two Cultures, and of his warning that the rift may grow wider unless the trend is checked.

Conclusion

Faced with these possibilities, what should we, as scientists, do? We are in some sense a privileged minority group, and all of us should be ready to exercise the grave responsibility which we all share, "to increase public understanding and appreciation of the importance and promise of the methods of science in human progress." These words are quoted from a statement of the objectives of this Association. A second objective of our organization is "to improve the effectiveness of science in the promotion of human welfare." These two should be the articles of our scientific creed in the years ahead. Furthermore, as scientists we should not lose our perspective but should recall the history of science and remember that it has survived pestilence, wars, and disaster and has surmounted barriers of race, religion, and language. Beyond this, it is even more important to recall, in a gray period of international tension, that all members of the human race, throughout its evolution and long history, have had a common opponent. This is inscrutable nature with her seemingly inexorable laws, her hosts of organisms and parasites, her hurricanes and catastrophic events of all kinds. For our human race the central problem is still that of understanding nature and attempting to control it. Here the thinking and tools of modern science have a great contribution to make. May we use them well.

Much of what I have said of warnings, of impacts and reactions, and of grave concern may have the ring of pessimism for the future as science moves swiftly ahead in one of the great adventures of the human mind. That this is not my intent can be made clear by a closing quotation from Carlyle's great satire *Sartor Resartus.* In this he attributes to his fictitious author, "Professor Teufelsdröckh of Weissnichtwo," these words, in the promethean spirit of which I share: "Man's unhappiness, as I construe, comes of his Greatness: it is because there is an Infinite in him, which with all his cunning he cannot quite bury under the Finite."

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BIOPOLITICS: SCIENCE, ETHICS, AND PUBLIC POLICY

By LYNTON K. CALDWELL

LAST year a front-page column of the New York *Herald Tribune* carried a whimsical description of a new science of biopolitics. J. P. Miller, already secure in his reputation for social criticism through satire in *Days of Wine and Roses,* recounted an imaginary interview between an official government biopolitician and a newspaper reporter concerning the meaning of the "new science" of biopolitics, "the science of proving that what must be done for political reasons is biologically safe for the human race."

The reported interview occurs sometime after 1971, when the collapse of the nuclear test ban treaty has been followed by a resumption of massive testing in the atmosphere and soaring levels of fallout. In order to relieve popular fears and prevent panics and anti-government demonstrations, official biopoliticians "prove scientifically that the previous human tolerances to radioactivity and all other by-products of nuclear testing, including strontium 90, had been estimated far too low." The official pronouncement has "a wonderful calming effect on the people." Public confidence is restored.

But, asks the reporter, suppose that an increase in bone cancer is being caused by heavy concentration of strontium 90 in human and animal marrow? Some unofficial scientists say so. But the official biopolitician replies that statements which frighten people are certainly not in the public interest. Bone cancer and strontium 90 cannot be linked, he declares. "The people wouldn't like it. Therefore, by definition it is biopolitically impossible."

In the tradition of the moralizing fable, Miller is posing one of the biggest, most difficult questions of our time: are science and politics really compatible? The philosopher-dramatist with a sociological turn of mind can put the question this way. Presumably the political scientist could too—but he rarely does. As "scientist" he finds it impractical to ask questions about the extent of man's political capacities that the present state of knowledge does not permit him to answer. Moreover the discipline of political science in America has, in its subconscious, assumed the infinite perfectibility of man. To hypothesize that political man cannot or will not reshape his goals and values in the light of scientific knowledge seems disloyal to the tradition of the discipline. But while the question cannot be usefully posed in absolute and theoretical terms, it is by implication being posed daily in limited and practical situations. In the language of politics "it is a condition that confronts us, not a theory."

An explosion of biological knowledge and technology is raising questions of public policy which until recently were hypothetic, and were therefore from a practical point of view unreal. Whether there is, can, or should be in any sense a science of "biopolitics" can easily be dismissed as facetious. But the conscientious man grows uneasy when he reflects upon the mounting problems which the life sciences (in particular) are posing for political solution. There is certain to be more biology in politics and this could mean, as J. P. Miller implies, more politics in biology.

The scientist, the politician, and the philosopher, each in his own way, is confronted by the question of how political reactions to an expanding, innovating biology will affect its application to the public happiness and welfare. And unfortunately for the policy-makers, happiness and welfare do not always follow from the same course of action. Yet there are urgencies in our present "biopolitical" state of affairs that compel a reconciliation of ethical values and scientific facts in public policies involving the biological nature of man.

"Biopolitics," then, though it certainly does not designate a science, is a useful piece of shorthand to suggest political efforts to reconcile biological facts and popular values—notably ethical values—in the formulation of public policies. It affords a selective focus on a portion of the larger issue of the relationship of science to society.

For several decades, spectacular developments in the physical sciences have overshadowed major but less readily demonstrable advances in biology. Moreover the impact of applied biology upon society often occurs on a time scale that obscures its effects—at least in the early stages. Thus the present population explosion has been underway ever since public health administration and medicine began to eliminate the "natural" controls over human reproduction. The explosion of population may be as inexorable and destructive as the explosion of nuclear energy, but the consequences of the nuclear bomb are all too readily observable whereas the potential consequences of the population bomb are inferred through the dry and less convincing medium of statistics.

Although there is widespread and profound disagreement as to its implications, the population explosion is now generally acknowledged. There is less awareness of a concurrent explosion of biological knowledge, an accelerating geometrical expansion of knowledge, the culmination of long years of accumulating inquiry in the various bio-sciences. It is the contemporary convergence of these two explosions—of people and biology—that justifies, indeed necessitates, a focus on biopolitics.

If the popular press and political behavior are taken at face value, people are nowhere (certainly not in America) ready to cope either conceptually or politically with the population explosion. This circumstance in itself is a major element in a larger body of evidence suggesting the unreadiness of most peoples and their governments to deal effectively with an impending explosion of biological knowledge. That extraordinary advances in biological science and biotechnology are imminent seems certain. To this there has been informed and responsible testimony for some time. Detlev W. Bronk, President of the Rockefeller Institute, has stated that ". . . we have learned more about the nature of living matter and the mechanisms of living organisms during recent years than in all prior human history." And the rate of learning accelerates. The revision of man's perception of himself and of nature that the biological sciences may require could be as drastic as the changes made by the physical sciences in man's perception of the cosmos. William K. Wyant, Jr. recently noted the likelihood that "the rough jolts of the future, in the way man thinks of himself, will come from studies done with the microscope."

The more sensational speculations growing out of biological congresses make news headlines and sober editorials. Commenting on the unprecedented implications of the emerging biotechnology discussed at the Eleventh International Congress of Genetics, an editorial in the New York *Times* declared that "the moral, economic and political implications of these possibilities are staggering" and then asked rhetorically "is mankind ready for such power?" In the judgment of some of the most thoughtful students of man's biopolitical behavior the answer is "No." Representative of misgivings in the scientific community is the regretful observation of Theodosius Dobzhansky that man, comprehending the meaning of his biological evolution:

> . . . should be able to replace the blind force of natural selection by conscious direction, based on his knowledge of nature and on his values. It is as certain that such direction will be needed as it is questionable

whether man is ready to provide it. He is unready because his knowledge of his own nature and its evolution is insufficient; because a vast majority of people are unaware of the necessity of facing the problem; and because there is so wide a gap between the way people actually live and the values and ideals to which they pay lip service.

Public unreadiness to use an expanding biotechnology wisely is not merely a speculative conclusion. Popular behavior and political action (or inaction) indicate prevailing attitudes toward biological realities. A cursory look at some of the current biopolitical issues suggests a mixed and contradictory picture. In each case a confrontation of biological facts, political exigencies, and ethical values occurs in the course of policy-making.

Biopolitical issues tend to fall into two general groups differing chiefly in the directness and generality of their effects. The first group may be termed environmental. Issues in this category arise when environments are impaired as a consequence of deliberate or inadvertent human action. The most dramatic of these concerns radio-active fallout. The attendant confusion of counsels and political recriminations hardly need comment. Whenever biological innovation is believed to threaten public health and happiness, and when scientific evidence can be marshaled in support of opposing views, a biopolitical row is inevitable. The fluoridation controversy, chronicled recently in the *Saturday Review*, is a case in point. Another is the danger of chemical poisoning through pesticides, dramatized by Rachel Carson's *The Silent Spring*, which engendered controversies described by René Dubos as ". . . disgraceful both from the scientific and social points of view."

Biopolitical controversies, frequently as heated, have arisen over efforts to conserve scientific and esthetic values in natural landscapes and in plant and animal wildlife. More recently questions concerning the effects of noise and of crowding upon human populations have been pressed forward. But in none of these matters has public policy making been pursued with the vigor urged in most of the polemic and some of the scientific literature. Perhaps this is because a clear and unequivocally right course of action seldom emerges from the research findings and the contradictions of scientific and of popular opinion.

For this failure to deal effectively with environmental problems the scientific community bears some responsibility. In a recent critique on environmental biology René Dubos takes his fellow scientists to task for gross neglect of ". . . the problems posed by the response of the total organism to the total environment." He argues that the potentialities of medicine for human welfare will be severely restricted until medical science has been provided with adequate scientific knowledge of "the effects of the total environment on the human condition." When scientists themselves offer no adequate explanation of the responses of body and mind to the impact of modern technology, has the politician any choice other than to trim biological facts to fit political circumstances? If science cannot speak authoritatively regarding the threats to physical and mental health posed by "constant and unavoidable exposure to the stimuli of urban and industrial civilization; by the varied aspects of environmental pollution; by the emotional trauma and often the solitude of life in congested cities; by the monotony, the boredom, indeed, the compulsory leisure of automated work," how can the politics of these issues be guided by science? It may indeed be argued that science, and biology in particular, are providing society with a powerful array of tools and problems, but with no adequate conceptual basis for relating tools to problems in practice.

A second group of biopolitical issues are more directly and specifically physiological than environmental. More personal in immediate impact although scarcely less general in ultimate ramification are biopolitical issues relating to individual human behavior in the use of cigarettes, tranquilizers, narcotics, and alcohol—and extending to the biochemical control of personality. Even more personal and at the same time of greater social implications are questions relating to human reproduction, to social concern for the numbers and qualities of future populations. In addition, ethics and biology become mutually involved in the political issue of public responsibility for public health and medical care. And finally the relations between biology, politics, and ethics are perhaps most starkly posed in the issue of biological warfare. In few of these areas have people demonstrated a readiness to be guided by verifiable knowledge in a search for policies equal to the problems. On many matters, inadequate as our knowledge may be, our failure to make full use of what we do know is all the more regrettable.

Biopolitical problems—particularly the major ones—grow increasingly national, international, and even global in character. The continuing flow of air and water and living organisms around the world has always tended to spread biological phenomena into any receptive environment. Modern technology multiplies and accelerates these possibilities, but it also enables us to discover and to understand the processes of dispersion and interaction. Where cause-and-effect relationships in these processes have become clear they have sometimes influenced political behavior as, for example, when the sciences of epidemiology and plant pathology led to the establishment of quarantines at national frontiers and were among the factors leading to international cooperation in public health and agriculture. Continuing difficulty in controlling international traffic in narcotics and the recent tragic consequences of the sale of the dangerous drug thalidomide in international commerce underscore the lesson that there can be no biopolitical frontiers.

A convincing argument can now be made that old-fashioned political nationalism is one of the principal obstacles to biological sanity. How much positive harm or deprivation may a nation lawfully inflict upon the rest of the world in pursuance of its alleged "sovereign rights"? Atmospheric testing of thermonuclear devices has posed the question dramatically, but a list of other major biopolitical issues, current and impending, could be extended to great length and in great variety. Obvious illustrations are found in national policies pertaining to the destruction of wildlife, allocation of water from international rivers, disposal of harmful wastes, control of plant and animal diseases, and increase in populations.

The inadequacy of conventional political mechanisms to deal with the problems of the new age of biology is nowhere more apparent than in the oceans from which life may well have come and from which man is increasingly drawing sustenance. As knowledge of the influence of the oceans upon terrestrial life continues to grow, so too does apprehension concerning impairment of their life-sustaining qualities. Massive discharge of untreated biological and industrial wastes into rivers, lakes, and coastal waters has impaired or destroyed important resources of food supply and recreation; residues from oil-burning seacraft have been so harmful to marine life that international control efforts have been sought; and proposals to bury radioactive wastes in the sea have aroused fears and controversy. But deliberate pollution is not the only problem. The Surgeon-General of the United States Public Health Service reports that the insect-killer DDT in some mysterious manner has invaded the water environment of the world and is being found in surprisingly large concentrations in the fats and oils of deep sea fish.

But the most portentous biopolitical issues relate to the evolution of man himself. The coincident and related explosions of human population and of biological knowledge may conceivably represent the most critical stage in human evolution since the last great ice age. The ability and necessity to control the numbers and hence (in some respects) the genetic characteristics of future populations could create a situation without precedent in human existence. And, in addition, the availability and refinement of chemopsychiatric drugs suggests both hoped-for and frightening possibilities for the manipulation and control of human behavior. Never before have the necessity and the possibility of control over man occurred at so decisive a conjunction.

Popular (and political) "wisdom" tends to avoid facing issues in advance of a compelling necessity. Questions as sensitive and confused as those just mentioned are especially good candidates for relegation to some indefinite future. But if society's ability to deal effectively with a problem requires policy decision *before* the matter becomes a compelling issue, then some means must be found to enable political action to anticipate the future. Practical biopolitics calls for a degree of foresight that the lexicon of conventional wisdom would term "theoretical." And practical democratic politicians find it difficult to persuade themselves or their publics of the necessity of dealing with tomorrow's uncertain problems when the self-evident issues of today press for attention.

At the root of these issues one finds the familiar dichotomies: fact and value, science and tradition, knowledge and action. If society moves ever more rapidly into an age of biology, how well can public leadership—scientific, educational, and political—bridge the gulf between the realities of popular concepts and the realities of scientific fact? If a massive reorientation of popular attitudes would be necessary for society to benefit fully from the *present* state of biology, how much more orientation may be required to develop a popular receptivity to the biology and biotechnology of the emerging future? There are wide gaps to be bridged between the biological sciences and public policies, and present resources are not adequate to the task.

The building of a better bridge between science and society leads to consideration of four basic elements in the process. These are: first, prevailing perceptions of man's relation to nature; second, the meaning of science as interpreted by formalized education; third, communication between scientists and policymakers; and fourth, leadership toward a policy synthesis of scientific knowledge and ethical values. Whatever utility the concept "biopolitics" possesses is primarily in relation to this fourth element. But all four are ultimately interrelated.

It is commonplace that man's perception of himself in relation to his environment is influenced by his culture pattern. In cosmopolitan and dynamic societies, these perceptions may range widely, as they have for example in the history of the American people. But in the realm of politics and social policy, some perceptions prevail over others. And, with acknowledgment of the inevitable exceptions, it is generally true that man's perception of his environment has in the main been possessive, exploitative, and short-sighted. From science, society has more often sought technology than understanding. The eminent ecologist Paul B. Sears has said, "The power of applied science has been overwhelmingly employed to exploit space, while those aspects of science that could illuminate its wise and lasting use are still largely ignored."

Industrial man (which until recently meant Western man) has for the most part seen himself as separate from and outside of nature. From this inference he has frequently concluded that he may exploit nature with impunity and that where nature fails to meet his wants, science through technology will synthesize a substitute. As *The Wall Street Journal* optimistically editorialized with respect to man's insatiable needs: "Technology, as always, can serve them." There has also been in Western civilization a perception of man *in* nature and a belief that he should seek understanding of his true needs and welfare through science. But this has been a minor current in a mainstream that uses science as servant rather than as teacher.

How science is used depends in large measure upon how its meaning is interpreted in the processes of formalized education. Science has been a potent influence upon education, but educational theory and practice have also shaped the course of science. Today more than ever the development of science depends not only upon the amount but also upon the nature of the incentives and support accorded it in the educational structure. For example, progress in fields as apparently diverse as medicine, human relations, and city planning is currently retarded because of past neglect of the environmental sciences. Interpretation of the implications and the needs of science to educators and to the public at large therefore becomes a crucial element in the advancement of science as well as of society.

The expansion, specialization, and diversification of biology and of all other sciences multiplies the difficulties of communication. New sciences create a need for new syntheses to relate and interpret their findings. New interdisciplinary areas take shape to deal with the new questions emerging between diverging sciences. In time, many of these interdisciplinary areas develop into coherent disciplines—into new sciences—and the process of specialization and of divergent and emergent disciplines continues.

Throughout this process direct and meaningful communication between the highly specialized research scientist and the public-policy-maker becomes ever harder to achieve. Popularizers of science have appeared in response to popular need and interest. But their status is as uncertain as their role is difficult. The best of them may find careers in journalism and may win recognition among scientists for informed and competent reporting. But there is at present little room for them in the structure of formalized education even though the need for better communication between science and the rest of society is now widely recognized.

The problem of how to organize this communication is yet to be solved. This is perhaps because communication is not merely exchange of information. And information is itself more than mere data; it is data plus meaning, intended and understood. The possession of scientific knowledge holds no promise of its use in discovering the true needs of men or in serving the public happiness or welfare. There is need for more knowledge but even greater need for more understanding.

Development of valid and coherent concepts of man-in-nature requires an interrelating and a synthesizing of knowledge. It is a task of interpretative leadership. Committees of specialists may assist the clarification and integration of knowledge, but synthesizing insights and perceptions more often originate in the minds of individuals who only then can become the expositors, the interpreters, and the advocates of a new view of man and nature. This mediating role between science, ethics, and public policy may be filled in various ways by persons from varied backgrounds—from the sciences, from professional education, from philosophy, religion, or public affairs.

Among the more effective intermediaries between science and ethics in political life have been those public servants who have in their own ways been "biopoliticians" in the best sense. These men and women have not only seen a relationship between scientific knowledge and the public welfare, but they have acted on this insight to influence the course of public policy. One may cite as examples Harvey W. Wiley's crusade for pure food and drugs, Hugh H. Bennett's lessons in soil conservation, and Ira Gabrielson's labors to substitute science for folklore in the management of wildlife. In each of these instances and in more that could be cited, scientific knowledge, a fundamentally ethical perception, and skill in communication were fused in effective policy leadership.

Granted that some aspects of biopolitics rest upon solid scientific support, the fact remains that we have not yet laid down a comprehensive biological foundation upon which a "science of mankind" can safely be erected. The scientific basis of biopolicy is fragmentary and will most likely remain so until the need for a comprehensive, verifiable, conceptual foundation for a healthful, creative, self-renewing society is more widely felt than it is today.

Better popular understanding of the biological factors in society should follow from a more accurate popular comprehension of science in the broadest sense. George Gaylord Simpson has pointed out the integrating role of biology among the sciences: life is the phenomenon to which all principles of science apply. In certain specialized areas of biology, notably in relation to agriculture and medicine, there has been a continuous flow of knowledge from the laboratory to practical application. The histories of the agricultural extension service and of the public health movement in the United States afford cases in point. But the dual explosions of population and biology create a much broader need for the desirable kind of biopolitics that has been so effective in particular cases. To achieve this objective may require new machinery in government. More certainly, it will entail changes in the structure and content of formal education and the addition of new elements to the career development of teachers and public officials who in the long run are among the principal architects of public policy.

It is neither possible nor necessary to examine here the ways in which the machinery of government might more effectively promote and utilize scientific knowledge. Relations between science and government have been analyzed at length and are under study by several Congressional committees. There is agreement in principle that government must be adapted to the new conditions wrought by science, but less agreement on what changes should be made. The United States Public Health Service's proposed Center for Environmental Health illustrates how the growth of knowledge calls forth new agencies to extend and apply that knowledge.

The changes in education that are needed to bridge the gap between biology and politics are more clearly evident. Throughout the modern world communication and understanding suffer greatly from gaps in the structure of education—gaps that appear with specialization and with divergence among the sciences and between them and the humanities. But even C. P. Snow's pessimistic analysis of "the two cultures"—the sciences and the humanities—does not postulate a gap that is unbridgeable. And it can be argued that the structuring of knowledge in Western society is a major factor in the cleavage that he has dramatized.

There is perhaps no *one* best way to obtain a more adequate communication among the disciplines and a more effective integration of related knowledge. Among the older disciplines changes in concept and emphasis may be needed as, for example, in geography where the discredited "environmentalism" of the past generation is being replaced by search for a more valid basis for understanding man-environment relationships. We may also need new disciplines to interpenetrate the older ones—to give us syntheses—to provide the form and substance of a more comprehensive understanding of man and nature. The beginnings of answers to these needs may be discerned in some aspects of the behavioral sciences and in the emerging environmental sciences—some, such as ecology hitherto relatively neglected; others such as biometeorology, regional economics and outer-space environmental research, relatively new in concept and method.

If the conditions for a better biopolitics require more realistic popular perceptions of man-in-nature, one way to assist this popular understanding is through the re-education and training of teachers, public officials, and opinion leaders. Updating and improvement of the teaching of biology has been for some time a subject for attention by the American Institute of Biological Sciences. The development of an awareness and comprehension of the significance of scientific developments by persons *outside* the fields of science is a different although related problem. Both developments are needed in strengthening the foundation for an enlightened biopolitics.

An important but relatively neglected avenue toward broader public understanding of science is adult education in its various forms. There is special need for an interpretation of science in its most fundamental sense to be built into career development programs for executive officials in government, business, labor, and the professions. Science (and particularly the biological sciences) has heretofore received comparatively little attention in these efforts, possibly because the relevance of the sciences to most fields of career development has not been fully appreciated. If mankind is rapidly confronted by unprecedented possibilities growing out of biological research and by increasing difficulties resulting from increasing populations, the need for biopolitical reorientation may soon gain a general recognition that it does not now enjoy. But this task of reorientation will not be done well unless the implications of biology can be reduced to terms and concepts meaningful for public policy.

To bring about an up-dating of the biopolitical understandings of teachers and leaders in public affairs, a valid conceptualizing, interpretative educational leadership will be needed. Some of the leadership may come, as it has, from government itself. More will need to come from the universities, the learned professions, and research institutes. For it should not be inferred that biology offers ready answers to all the problems it defines or its applied technology creates. Closing the gaps of knowledge and restructuring that knowledge for attack upon new or persistent biopolitical problems will be, even more obviously than it has already been, a multidisciplinary task.

Some eminent scientists have shown skill in relating science to social needs and ethical values. But these extraordinary individuals have been too few and too infrequent to accomplish unaided the task of bridging the gaps between bioscience and biopolitics, between science and society. The sheer mass and specialized complexity of expanding knowledge create a need for a systematic and continuing effort toward synthesis from which intelligible conceptualization and communication may be forthcoming. As yet the task is barely attempted and then in only a few places.

Does all this then imply the need for a "science" of biopolitics for purposes quite the opposite of those suggested by J. P. Miller? The answer is both yes and no. It is no if biopolitics is understood *only* as a new formal academic discipline to deal comprehensively with social applications of biological knowledge. This is not to say that such a discipline is unnecessary—or would be impractical—or that it could not be developed. We have been concerned here with the problems and the needs suggested by the term "biopolitics," with general approaches to solutions rather than with specific remedial methods. But if a science termed "biopolitics" is not specifically implied, the need should be evident for a more effective relating of the biological to the social sciences and of both to public policy and ethics.

Without the interrelation and distillation of scientific findings into issues amenable to political action, the gap between science and politics cannot be successfully bridged. Science as technology may be readily available to the lower and more routinized levels of administration. But at the higher executive and legislative levels of the governmental hierarchy where the broad public policies are formulated, the science most relevant to the issues will be more conceptual than technical. The impact of scientific thought upon public policy will in large measure depend upon its being expressed in terms meaningful to political and administrative practitioners. The legislator and public administrator must make their own policy syntheses, but they can do their jobs more effectively if the data relevant to these decisions have been organized and reduced to understandable terms.

The case for aid to the administrator in his task of synthesis has been stated with exceptional clarity by Paul H. Appleby:

> Specialist after specialist pursues analysis; who pursues synthesis, or even pursues analysis with any sensible orientation to the larger function of synthesis? It is the synthesis which involves all the heavy burdens of practitioners, and these burdens are heaviest when the social action is most complex and most complexly environed. Synthesis becomes more and more important as one goes up the hierarchy, and more and more important as one moves from the relatively specialized fields of private administration to public administration.

This synthesis does not necessarily require new sciences. Ecology, for example, has long been an established if insufficiently utilized "organizing" science. Its further development and involvement with the social sciences could provide much of the needed synthesis. It also seems probable that new emphases will emerge in established disciplines, that interdisciplinary studies will increase, and that new formalized disciplines may emerge. New arrangements to facilitate interdisciplinary studies involving synthesis of the social and biological sciences and relevant professional fields—notably architecture, engineering, public health, and natural resources administration—are already under consideration in a number of universities. From these developments might come major contributions to the formulation of public policy in the years ahead.

"Biopolitics" therefore suggests a need that may be met in many different ways. It would be difficult to argue that existing educational resources are adequate. But because few educational needs can be shown to be fully served, the question will be asked: How important is this underdeveloped area of biopolitics in relation to other unfulfilled educational demands? Restating biopolitics in broader terms as study of the role of science in society, its priority is of the highest. We have been paying heavy and steadily rising prices in dollars, health, and happiness for its relative neglect, and have entered, inadequately prepared, upon a decisive test of our capacity to avoid becoming the victims of our own ingenuity.

The "condition that confronts us" calls for more than the mere tolerance of imaginative innovation in reshaping and accelerating the education of society. Tangible and timely encouragement is needed for pathbreaking efforts, for the ever risky tasks of synthesis, for the continuing development of creative individuals capable of conceptualizing and interpreting the issues that arise at the meeting point of science, ethics and politics. The study of biopolitics—whatever it may be called—requires an extraordinary fusion of understanding, audacity, and humility.

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Megaloscience

Because of massive organization and large budgets, scientists are heavily involved with governments.

J. B. Adams

I have chosen the title "Megaloscience" for this discussion of scientific research and its interaction with governments and universities in order to convey the impression of very large-scale scientific research with just a hint of underlying mania.

Scientists who have grown up with this activity and who are still involved in it cannot pretend to be unbiased, but we can try as objectively as possible to analyze the problems which our activities have raised and to find reasonable solutions to them. We must address our minds to these problems now, if only because governments have become very much concerned with scientific research. Partly their concern is due to the rising cost of research and partly it is due to a growing realization in political circles that scientific research and development are the mainspring of our type of civilization. This concern must ultimately lead to decisions being taken by governments, and if we are to take an effective part in the decision-making we must first clear our own minds.

Even if our thinking does no more than dispel that public image of scientific research so well summed up by Academician Artsimovich, "Scientific research is a method of satisfying private curiosity at the public expense," it will not have been in vain.

Limiting Scientific Research Budgets

To the man in the street the impressive thing about megaloscience is its apparently insatiable demand for money. Where it all goes and how it is used is a mystery to most people. What results come out are by and large incomprehensible to almost everybody, including even scientists in other fields of research.

To the astute civil servant a far more ominous characteristic is the growth rate of scientific activity. Ever since the 17th century, we are told, the number of scientists has doubled every 15 years and the cost of scientific research has doubled every 5 years. We should not, of course, accept these statements without some investigation, particularly on such points as the definition of scientist used in the statistics, but during my own professional lifetime these doubling times seem to be about right. Extrapolation of these growth rates gives the fascinating and unlikely result that all the national incomes of our countries will be spent on scientific research in the year 2000 and everybody will be scientists a few decades later. Clearly, between now and the year 2000 something must occur to limit the growth of scientific research, and our problem is to determine what the limit should be and how it can be reached without unstable oscillations.

Such figures as exist show that in countries such as the United States and

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Britain about 2½ percent of the gross national income is being spent on civil research and development. About a tenth of this, that is, ¼ percent of the total, is spent on research and the rest on development. These may appear rather small percentages compared with what is spent on seemingly trivial things such as alcoholic drink and tobacco, which between them account for nearly 10 percent, but unfortunately, if people want to spend 40 times as much on smoking and drinking as on scientific research, there seems to be very little that anybody can do to stop them. As we all know, a short life and a gay one still has its attractions, even to physicists. In my own country only about one quarter of the national income is directly spent by the government, and that goes on such items as military defense, national insurances, and other public services. If we are to determine some limit for scientific research expenditure it is probably more profitable to consider what governments do with their money than what the people at large do with theirs.

Now some of the larger countries spend as much as 10 percent of their incomes on military defense, and as a starting point it does not seem unreasonable to imagine that they could attain the same percentage for civil research and development. If the same fraction of this goes to scientific research as at present, namely one tenth, then the upper limit for scientific research would be 1 percent. Since we are currently spending ¼ percent and the doubling time is 5 years, it would only take another 10 years to reach this limit. But if we are to avoid oscillation we must approach the limit asymptotically by means of an S-shaped or logistic curve, and the exponential rise must stop at the halfway mark. In other words, we must arrest the exponential growth in 5 years' time, when the expenditures will have reached ½ percent of the national income, in order to approach the 1-percent level smoothly.

This is a very simple and perhaps naïve example, but it yields an important result, namely that if the limit is 1 percent and we want to avoid uncomfortable, if not disastrous, oscillations, we must take action in the next 5 years to stop the exponential growth of scientific research budgets. Even if the limit is 2 percent, we can only delay decisions another 5 years, and if it is less than 1 percent we must act very soon. All this is the result of the very fast growth rate of scientific budgets and it is the reason why I said earlier that we must think about these problems now.

The percentage figures I have been quoting come from published government statistics, but as we all know, there is considerable confusion in the definitions of the various forms of scientific activity, and certainly in my own country I doubt whether our present figures are a sufficiently reliable basis for action. For example, what is called scientific research, as distinct from development, is very ill-defined and differs markedly among the sciences. Also, I know of no justification for the present 1-to-10 ratio between research and development or whether this ratio should be perpetuated in the next two decades. In fact we know far too little about the whole matter, and it will take a year or so to gather reliable statistics, even given government support for national surveys. To decide on such a serious matter without these facts is surely unthinkable, at least for scientists.

Just in case my example strikes terror in the hearts of the military, I should add that the figures I have been using of 1 or even 2 percent of the national income for scientific research could easily be reached by steadily allocating, year by year, a small fraction of the normal annual increase in national incomes which most developed countries now enjoy—for example, the American gross national product is increasing at 4 percent per annum. Thus we do not need to abandon military defense in order to find money for scientific research, although if peace broke out it would be a way of absorbing military research-and-development potential into the economy.

The Organizational Scientist

Let me turn from these weighty matters and divert your attention for a moment to another remarkable aspect of megaloscience. I refer to group activity and multiple authorship of papers in scientific journals. This is particularly noticeable in the leading megaloscience of high-energy nuclear physics research, where the motto seems to be "United we publish, divided we languish." It is not only that papers have many authors but that the authors of a single paper come from many laboratories. For example, in one of the September 1964 issues of *Physical Review Letters* there are two papers, one from Brookhaven on the Ω^- hyperon with 31 authors, and the other from the European Center for Nuclear Research (CERN) on π^- meson interactions with nuclei, with 25 authors from 6 different laboratories in 5 different countries. I notice that one author of the CERN paper, the work for which was done in Geneva, explains in a footnote that his affiliation is Berkeley, California, although he is actually on leave of absence from Milan University.

Those of us who work in large laboratories know that the authors listed on a paper are by no means the only people involved in the work. The ratio of research physicists to total laboratory staff is about 1 to 7, so a piece of research with 31 authors involves on the average something like 200 people in the laboratory, and the whole effort costs the laboratory about £1 million a year. Usually what one gets for this large investment of men and money is just another small piece of a vast jigsaw. Of course one tries to plan the research so that it is a vital piece, but one cannot always be successful, and sometimes someone else puts the piece down first. Very often the vital pieces turn out to be cheaper ones and the stroke of genius which first delineates the whole pattern is usually the cheapest act of all. Nevertheless, without enough of the jigsaw pieces it is beyond even a genius to see the pattern, and so we must go on prising them out of nature, each one costing more than the last. I must emphasize that megaloscience is not different from other science in this respect; it is only that it is further along the exponential growth curve, where bits of information apparently cost more. How long we can afford to go on collecting them while waiting for a pattern to emerge is another question.

I have remarked earlier that the number of scientists has apparently been doubling every 15 years, ever since the beginning of modern science. I doubt whether the number of scientists of, say, the caliber of Newton, Einstein, Schrödinger, Rutherford, and Fermi is increasing at this rate, and if the growth of research budgets were dependent only on men of such high ability and deep insight, it is unlikely that the doubling period of 5 years in research expenditure could have been maintained in the last few decades. What seems to have happened is that megaloscience has maintained the growth rate in recent years, first by becoming highly organized, and second by making the maximum use of whatever genius naturally arises in any decade.

In fact two distinct types of scientist have emerged in this process of scientific evolution, the Manager Scientist and the Pilgrim Scientist. Whereas the Manager Scientist spends a great deal of his time in his own laboratory, the Pilgrim Scientist is rarely to be found at home. While the Manager Scientist is responsible for large groups of people and for large laboratories and is familiar with the ways of governments and treasuries, the Pilgrim Scientist eschews all such contacts and responsibilities. Indeed, he is more in line with the popular image of a scientist, and he goes around fertilizing research in many laboratories. The Manager Scientist is mainly a postwar phenomenon, although some existed before. His job is to create the conditions in which good research can be carried out, and his reward is seeing it flourish about him.

I have used the term Pilgrim Scientist because it suggests a parallel with medieval times. The medieval pilgrim had a definite itinerary—certain holy places and religious houses to visit on his pilgrimage—and his itinerary depended on whether he was a Franciscan or Dominican or belonged to some other order. He was also the bearer of news, religious and otherwise, as we can read in Chaucer. The modern pilgrim scientist also has his shrines and religious houses to visit, depending on his branch of research. In high-energy nuclear physics, for example, the equivalents of the old religious houses are Berkeley, Brookhaven, CERN, and Dubna. It is as rare nowadays to find a scientist attaining pilgrim status in more than one research field as it was to find a medieval pilgrim belonging to more than one order. And just as it was customary for the medieval pilgrim to be fed, housed, and looked after by the monasteries, so the modern research laboratory must set aside funds to pay foreign pilgrim scientists and to send its own on tour.

Perhaps in medieval times there was a problem with pilgrims settling down in particularly attractive monasteries. Nowadays any pilgrim scientist who is captured more or less permanently in a foreign laboratory is said to be part of a national Brain Drain. Luckily, drains were far less common in medieval times, so no doubt the medieval pilgrims were mercifully saved from that simile.

Of course scientists have always traveled around. For example, right at the beginning of modern science there was the case of Tycho de Brahe, the Danish astronomer. He traveled quite extensively in Europe and at one time planned to settle in Basle, where he found the scientific community most congenial. This did not please the authorities back at home and finally Frederick II, King of Denmark, sent him a letter—it is dated 23 May 1576, a few years after New York Bay was discovered by Verrazano—which reads as follows:

> We, Frederick the Second, make known to all men, that we of our special favour and grace have conferred and granted in fee . . . to our beloved Tycho de Brahe, Otto's son . . . our land of Hveen, with all our tenants and servants who thereon live, with all rent and duty which comes from that . . . to use, hold, quit and free all the days of his life as long as he lives and likes to follow his studia mathematices.

The land of Hveen was an island of 2000 acres on which Tycho de Brahe built a castle and an observatory at Denmark's expense, and, what with the sinecures and grants, he became one of the richest men in Denmark.

You will observe that this letter contains all the ingredients to stop a Brain Drain: promise of money and staff and, above all, the personal touch in the letter of appointment—"our beloved Tycho de Brahe, Otto's son." You will not find that nowadays, not even in offers from American firms.

To Choose and How To Choose

Let me return again to money matters. The notion that there must be a limit to expenditure on scientific research naturally raises the problem of choosing among the different fields. We who are committed to the megalosciences must necessarily consider this problem very seriously indeed. Dr. Johnson once observed, "Depend on it, Sir, when a man knows he is to be hanged in a fortnight, it concentrates his mind wonderfully," and, indeed, a recent exchange of letters in *Physics Today* on this subject shows a power of concentration. All sorts of criteria for making choices have been put forward, such as scientific merit, technological merit, and social merit, as well as the degree of fundamentality of the research. National prestige is also clearly playing an important role in this matter, and so is international competition. We may yet find ourselves involved in the Pythagorean Games, as our athletic friends are now engaged in the Olympic Games. After all, the cost of the Tokyo Olympic Games is about the same as the cost of a 300-Gev accelerator laboratory for nuclear physics, and we already have our Gold Medals.

But before we get too involved in this matter, I think it is essential to be clear as to the motivations of scientific research. To my mind there are two basic motivations; one is the desire to do something and the other the desire to know something. The first is the motivation of applied research and development, and the second is the motivation of basic research. Because the motivations are different, the criteria for choice in these two types of scientific activity are different and should not be confused. To illustrate my point I can take an example from my own subject of plasma physics and fusion research. The motivation of the work of the Culham Laboratory is to see whether or not a controlled thermonuclear reactor can be built. In pursuing this aim we will of course learn a great deal about the plasma state of matter—in fact we must, if we are to make progress—but this is not the motivation of the work and it is not the reason why the British Government is spending £4 million a year on the Culham Laboratory. Such scientific activities as these must be judged on the basis of how successful they are in reaching their goals, and choices among them must be made on the basis of the values of the different goals to the sponsors at different times. It is quite conceivable that a laboratory such as Culham could have been motivated by a desire to know about the plasma state of matter. In this case it would fall into the basic research category and would be judged on a quite different basis from and in competition with the pursuit of other knowledge, such as that sought through research in high-energy nuclear physics or molecular biology.

Because the motivations of applied research and development are different from those of basic research, the two activities are not directly comparable, and lumping them both together in a single research-and-development budget has caused a great deal of confusion, particularly at the government level. In practice it is probably easier for a country to decide what it wants to do than what it wants to know. What I shall now discuss is the second of these two dilemmas, namely, how to choose between the basic scientific researches.

My starting point is simply that basic research, as I have defined it, is part of scientific education. It is the pursuit of new knowledge about nature, and the other two parts of education are the preservation of this knowledge and the handing of it on to future generations. My thesis is that the three parts must be held closely together at all times because, once the unity of education is destroyed, I fear that the whole system will slowly but surely deteriorate. For hundreds of years this unity has been preserved by our universities, but the advent of megaloscience and the creation of large basic research laboratories remote from the universities can easily disrupt it. It was to counteract this danger that the founders of CERN insisted that the research physicists using that laboratory must not be given permanent contracts, since these would encourage them to settle down at CERN and cut them off from their universities and from teaching. To this day very few research physicists have permanent contracts at CERN—just enough to guarantee the scientific management of the laboratory.

This concept of the unity of education can also give us a rough way of judging the extent to which the various basic researches should be supported by a country at any time. Suppose, for example, we first determine the number of university scientists actively engaged in the different fields of basic research and then calculate the amount of money needed per year to maintain a research scientist in each field at maximum efficiency. Obviously the cost per research scientist per annum is not the same in all research fields—it depends on the scale at which operations have to be conducted. At the megaloscience stage, for example in high-energy nuclear physics, it costs about £30,000 a year to maintain a research physicist efficiently, and it is rather a waste of money to maintain him otherwise. This figure is obtained by taking the total annual budget of a laboratory, such as CERN, and dividing it by the number of research physicists working in that laboratory. Other basic research fields not needing such large equipment cost less per scientist. The basic research budget is then composed by multiplying the cost per scientist by the number of active university scientists in each field, which gives the individual budgets for each research field, and then adding the lot together to give the total budget. At least this system of determining basic research budgets is constructive and avoids subjective judgments about the relative merit of the various research fields. Surely in trying to determine what a country should know it is safer to base the support on what its active research scientists find most challenging and worthwhile and to which they are prepared to devote their lives.

Sooner or later, of course, the total basic research budget calculated in this way will exceed the limit which I discussed earlier, and this is likely to happen first in the most developed countries. We must therefore consider what will happen in countries which have not yet reached this limit and which are making available less money for basic research than is calculated by the method I have just described.

The first reaction of an active research scientist who cannot obtain the necessary research facilities in his own country is to seek them elsewhere. Thus the first result of a financial limitation of basic research is the emigration of research scientists—a phenomenon with which we are only too familiar in Europe. A study of the pattern of scientific emigration can give clues as to what is wrong with the support for the basic researches. For example, if the emigration is confined to scientists in one field of research, it probably means an unbalance in the distribution of funds. If it covers all fields, then the total funds are probably inadequate in comparison with those provided by other countries. In my experience scientists do not emigrate for trivial reasons, and it takes several years of neglect to drive them that far. Thus the emigration figures are at best a very delayed manifestation of an unbalance.

The serious consequence of scientific emigration is not that a country cannot obtain the results of basic research, for they are all published and available to anybody. It is that fewer active scientists are available in the country to teach and inspire the next generation of scientists and the whole system of scientific education begins to run down. Hence my insistence on the importance of the unity of education. Also, since the best scientists can most easily find jobs abroad, the damage to the education system is far greater than the numbers emigrating indicate.

Clearly this emigration only continues so long as one country is further along the exponential curve of scientific expenditure than the others, and ever since the war the attractive country in this respect has been the United States. However, it is reasonable to suppose that that country will reach the limit of expenditure on scientific research first and so give the other countries the opportunity to catch up. In other words, scientific emigration need be only transitory if countries recognize its causes and try to reach the common limit as soon as possible. Nevertheless, in the megalosciences the absolute size of a country, and therefore the size of its investment in basic research, becomes important. For example, in high-energy physics the sheer size and cost of modern multi-Gev particle accelerators make it impossible for a small country to build them alone, however advanced that country may be in its support for science on a percentage basis. The solution in these cases is for a number of countries to combine together in a joint project, as was done in the case of CERN for high-energy physics. The advantage of CERN, quite apart from its contributions to physics, is that European high-energy nuclear physicists no longer have to emigrate to America in order to continue their research and hence they tend to remain in Europe as a vital part of its scientific education. Ultimately, as the cost of individual pieces of equipment in the megalosciences mounts, even the largest countries or groups of countries will be driven to unite if the research is to continue, and this is already being discussed for the 1000-Gev stage in high-energy physics.

It might be thought, and I have seen it proposed, that the smaller countries should use their limited resources for applied research and development and give up basic research, particularly at the megaloscience level. I think this notion is as dangerous as it is tempting to such countries. The active and original minds in science in these countries will not be satisfied with technology and applied research and will simply emigrate, thus reducing the standards of scientific education to a level where even the quality of applied scientists and development engineers may become inadequate for their tasks.

I have also heard the allocation of funds for basic research described as "dividing up the national cake." As I have tried to show, the method of determining research budgets should be additive, not divisive. Research budgets should be built up from the ingredients of research, which are the active scientists, and their proportions should be determined from what such people find most challenging and worthwhile in research. Also, basic research is not cake—it is bread and the staff of life of our type of civilization. We must get away from the idea that basic research is only a cultural activity or that its value to the community is some kind of "fallout" in technology and industrial processes. Of course our modern technology is a direct result of past basic research. The electronics industry is a result of J. J. Thompson's discovery of the electron, and the nuclear energy industry is a result of Rutherford's discovery of the nucleus. The cost of all the basic research that has ever been done is barely equal to the current year's increase in the gross national product of the larger countries, and without all that research it is doubtful whether they would now be enjoying any increases in prosperity. Nevertheless it is difficult to use such arguments for planning research expenditure in the future, however completely they justify research in the past. The true place of basic research is as a part of scientific education, and no industrial country these days can afford scientific illiteracy, whether it be in its universities, its industries, its government, or its people. We must therefore seek our guidance from this latter connection and merely accept the former as the natural consequence of enlightenment.

In Conclusion

Let me now try to draw together the threads of my discussion into some simple statements. I believe, for the reasons I have given, that we must be within a few years of the end of the exponential growth in scientific research which started in the time of Kepler, Galileo, and Newton and has been going on steadily during the last 400 years. Up till now this growth has been free and similar to the increase in populations which are not severely limited by food supplies or disease. I believe that we as scientists have an important part to play in the next most difficult phase in the growth of our subject, which is to bring the exponential phase smoothly toward a limit without oscillation or discord. We must use our skills as scientists on the growth of science itself.

I have mainly discussed basic scientific research as a vital part of the whole of scientific education. Even in the applied researches with definite goals which are supported by our countries because of these goals, we must continually examine our purposes. In nuclear fusion research, for example, we must be sure that nuclear fusion reactors remain worthwhile to the community and that we are making progress toward their realization. At the moment I think they are worthwhile and that we are making considerable progress, but if the time ever comes when their value is minimal and our progress questionable, I hope we will have the courage to speak out first and not wait until other people outside science find out and take appropriate action. As you know, the reason why populations do not continue to grow exponentially is that they either become diseased or exhaust their food supplies.

Megaloscience as the last phase in the long history of the growth of modern science has certainly brought a great number of problems for our generation, but it is only fair that I should end by briefly mentioning some of its less obvious blessings. Like technological fallout, they are perhaps incidental and were certainly not foreseen, but they have their importance.

Simply because it has grown so big, megaloscience has caused countries to act together in joint enterprises which, owing to their nonpolitical nature, have enabled methods of international behavior to be worked out far more quickly than has been possible in more controversial fields. CERN is a very good example of what international cooperation in scientific research can offer to small countries like the European states. The massive organization and large budgets of the megalosciences have brought scientists into headlong involvement with governments and treasuries, and although this interaction has not always been blissful, I think everybody has benefited from it. Also, the Manager Scientists and the Pilgrim Scientists have certainly opened up new channels of communication between the nations which even in nonscientific matters have remained remarkably direct and effective.

Perhaps future generations, looking back at our struggles with the growth rates of science and the limits, may well rate these incidental achievements as highly as our research results, and in terms of human welfare they may even find them to have been of greater significance.

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The Changing Environment of Science

What are the effects of the so-called scientific revolution upon science?

Alan T. Waterman

I suppose it is fair to say that scientists as a class have deeper concern about the present state of the world than most groups. This concern is natural and understandable. It undoubtedly had its origin in World War II in such dramatic developments as the atomic bomb and biological warfare, which disclosed new and awesome possibilities of man's destroying himself through the findings of scientific research. Since that time the picture has broadened and changed considerably. With the cessation of active warfare the contribution of scientists to the development and use of military weapons and devices has lost much of its compulsive quality and has returned to a more normal state.

However, the change of greatest import to scientists developed as an outgrowth of the cold war. It is the general realization that the entire future of a country, not just its military might but its economic strength and welfare, depend markedly upon its progress in science and technology. This has brought scientists into prominence as the potential saviors of their countries, a most embarrassing position for any group but especially for ours. In the past we have tried to avoid publicity; it is a disturbance to calm and concentrated thinking. Rightly or wrongly, approbation of the public has been of relatively little importance to us; it is the recognition and respect of our colleagues that counts. In normal times our allegiance is strongly to our science; to attempt to direct our efforts toward causes of national importance is ordinarily confusing and disturbing. We have a fundamental conviction that the country's cause is best served when we are given *carte blanche* to work as we see fit, since we know very well that the greatest progress in science is made when that is the case. But this is an oversimplification. We must admit that the demands of technology have been present from the very beginning. Archimedes produced his engines of war, Galileo studied the operation of well pumps, Newton gave serious attention to navigation, Jenner had his cow pox problem and Pasteur his beer project and the prevention of infection. And, as a matter of fact, such pressures have been responsible for much important progress in science itself. For some time now the feedback from technological innovations, themselves stimulated by basic research, has made countless techniques and instruments available for fundamental research.

Thus, in an important sense the conflict between science and technology is not in itself a real conflict of interest. The conflict occurs principally because of the competition between the two for money and manpower. The public, unable to distinguish clearly between them, gets into the act by insisting upon prudent, economical, and understandable use of its (public) funds—that is, tangible and useful results. Unfortunately for many academic scientists, life has a way of presenting insistent practical problems whose solutions require their attention.

Within science itself, however, one finds an increased sense of responsibility for our future. This is not due solely to the strengthening of the popular image of the scientist; it is heightened within science itself—in many areas which give high promise of progress, notably in biochemistry and genetics; in nuclear physics, with its potential for power from nuclear fusion; in exploitation of the oceans for food and minerals; in attempts at weather modification, and in the exploration of outer space. This promise is augmented by the extension of knowledge to such incalculably remote domains as the atomic nucleus and galaxies nearly at the boundary of the known universe, not to mention the almost unbelievably complicated structure and behavior of the living cell.

This sense of responsibility is spurred by the reputation scientists have acquired with the general public, which has served both as a stimulant and a sobering influence. On the one hand every country has come to believe that its salvation lies in technology—usually misnamed science. The technical industries have looked to their research departments and their research analysts for the most dependable forecasting of profitable lines for future development. The man on the street seems to view all this with mingled feelings. On the whole he is grateful for progress in health measures, communication, housing, transportation, and the many technical conveniences which both simplify and complicate his existence, despite occasional feelings of resentment over the increasing novelty and complexity

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This article is adapted from the presidential address delivered by Dr. Waterman, retiring president of the AAAS, on 28 December 1964 at the Montreal meeting.

that surround him. When it comes to major events, such as possible devastating nuclear war, fallout, or the costly but intriguing conquest of space, the element of anxiety is added. "Why isn't there some way," he asks, "to keep our technological advances within safe and prudent channels? Why must these troublesome questions arise? Can't scientists be persuaded to work only constructively? If they can't do that, why don't we limit their activities by providing money only for selected desirable and noncontroversial enterprises?" Indeed, some would go so far as to advocate a moratorium on research in the natural sciences, on the one hand to avoid such disagreeable issues as those posed by nuclear and biological warfare and, on the other, so they say, to allow the social sciences to catch up and solve these vexing problems before the natural sciences make them too tough.

Science Policy

In the meantime, how has modern society been dealing with all this? What degree of attention are nations giving to this subject? To what extent do their governments participate in the conduct or support of scientific research and development? What sort of policies are emerging? These are questions which the Organization for Economic Cooperation and Development (the OECD) has canvassed among its member countries, a task for which we owe it a debt of gratitude (*1*).

In the OECD observations regarding general aspects of science policy, the following points were apparent. In the first place, education is a critical factor, since it must provide the human resources for technological progress and because it creates a favorable psychological climate. Next there must be up-to-date provision for the numbers and skills required in the labor force. An important consideration is the training of potential research workers, and especially of future managers. It appears to be generally agreed that scientists and engineers of high capability are desirable in management positions, in both industry and government. Finally comes the training of the research scientists and engineers themselves. This necessitates high quality in the graduate and postgraduate facilities at universities and institutes of technology.

Immediate potential for meeting these criteria of course exists in a number of countries. In others, further development is required, and in general this becomes a responsibility of the respective governments.

All countries face the problem of securing a satisfactory output of trained scientists and engineers. In most countries this problem is caused by a very rapid rise in research and development expenditures over the past decade; this rise greatly exceeds the increase in the gross national product (*2*). Generally speaking, the ratio of R&D to the per capita GNP is high (1 to 2 percent) in the large industrial countries; in these, industry performs two-thirds or more of the R&D. In countries where there is strong emphasis upon agriculture, forestry, mining, and fishery—such as Australia, Finland, Canada, and Norway—the ratio of R&D to the per capita GNP is lower and industry performs only about one-third of the R&D. In almost all countries the government finances most of the R&D (*3*).

In practically all countries fundamental research is primarily conducted at universities and nonprofit institutions and is increasingly receiving support from government. In the United States, the United Kingdom, and France the government finances applied research and development largely through contracts with industry. Such financing occurs to a much lesser extent in Canada, the Netherlands, and Japan. In Canada, I understand, nearly half the total R&D is performed in government laboratories.

Whereas earlier, in most countries, provision for R & D funds was chiefly the responsibility of the Ministry of Finance or the equivalent office, the more advanced countries are giving increasing attention to the formation of top advisory councils to their governments, which work in cooperation with the finance office. Likewise, in the more advanced countries the influence of the national academies of science has grown, in providing advice to government and in setting the national and international tone for scientific achievement.

Such is the situation facing us and other nations at this stage of development, and such are some of the ways in which we and they have moved to meet our problems.

Because of the mounting commitments to science and technology, much talk and some concentrated thought and study have been directed toward improving the effectiveness of our efforts through planning, management, and education. There have also been attempts to evaluate the impact of the national effort upon our economy and our national achievements. These are important questions, and it is earnestly to be hoped that the current efforts will stimulate concerted and continuous study among economists, social and natural scientists, educators, and administrators. The issues are complex and are not likely to be solved by *ad hoc* committees or conferences alone.

Insofar as this activity sharpens the focus of our attention upon the identification and definition of worthy objectives, their relative priorities, and the feasibility of proposed means of achieving them, it must be regarded as very worth while. However, two important caveats should be heeded:

1) The planning for the identification and pursuit of technological objectives, no matter how feasible or worthy, should not be permitted to monopolize the national effort at the expense of science, and of basic research in particular. Such a policy leads in the long run to diminishing returns and ultimate stagnation.

2) Any attempt to forecast detailed money and manpower requirements for free research in the component scientific disciplines is, in my opinion, a questionable undertaking, no matter how experienced and distinguished the reviewing body. Applied research will always receive this kind of attention. But such attempts for free research introduce a concerted extrapolational bias into the system and sound an authoritarian note. Besides, what stronger motivation can there be for creative, original research than the individual scientist's own evaluation and decision as to the most promising course for him to pursue? As history abundantly proves, the capital discoveries in science generally lie in the unknown and cannot be predicted or planned for—and these may occur in any branch of science.

Effects of Changed Environment on Science

I shall not pursue further this topic of the impact of science and technology on society. Rather, my purpose is to invite your attention to the effects of this radical and sweeping transformation of activity upon the progress of science itself, stressing science in its traditional sense of the "search for truth." In the dynamical center of this interpretation lies basic research—the systematic and specialized search for knowledge and understanding. But of course science is more than this; it is the organized and classified body of knowledge which results from the search. Research is merely its frontier.

With the recent universal recognition that science and technology are essential to the progress of civilization, and with the attendant glamor which attaches to research, the environment for science has altered. For most of its history the devotees of science have been attracted to its study not primarily for the purpose of securing information that might be useful in some practical way but, like Kipling's elephant's child, out of "satiable curtiosity," in the search for new knowledge no matter where or what it might be. With this motivation dominant, the search for knowledge in science has proceeded without boundary or limit. Scientific exploration mushroomed out in all directions, encompassing a range which would have been impossible under concerted planning. Of course many extremely important advances occurred by reason of some practical need or incentive, but by and large the scope and range of scientific investigation was not dictated by such considerations.

During the present century the technical industries, whose existence depends upon successful practical development and production, have increasingly come to conduct research themselves. Many of them have even recognized the advantages of pursuing basic research in areas where such work will lead to better understanding of their technological problems. This trend was accelerated during and after the war by such sensational results of research and development as atomic energy, radar, and the transistor. Nowadays no progressive technical industry or government bureau would attempt a large developmental enterprise without careful survey of the underlying research and, where necessary, inclusion of such research as part of the developmental process.

Mission-Related Basic Research

However, a larger proportion of support is provided for applied research than for basic research; in the U.S. the ratio is about 2 to 1. There is a corresponding majority of scientists employed by industry and government as compared to academic and other nonprofit institutions. For engineers the ratio is of course much higher than it is for scientists.

But this is not all. There has been a steady increase in the support of basic research which may be termed "mission-related"—that is, which is aimed at helping to solve some practical problem. Such research is distinguished from applied research in that the investigator is not asked or expected to look for a finding of practical importance; he still is exploring the unknown by any route he may choose. But it differs from "free" basic research in that the supporting agency does have the motive of utility, in the hope that the results will further the agency's practical mission. A considerable body of basic research is receiving support because it is so oriented. Thus, basic research activity may be subdivided into "free" research undertaken solely for its scientific promise, and "mission-related" basic research supported primarily because its results are expected to have immediate and foreseen practical usefulness. Much of the emphasis upon basic research in the areas of cancer and solid-state physics illustrates "mission-related" characteristics. Since the support of "mission-related" research is easier to justify, when budgets become tight it tends to survive at the expense of "free" research. This tendency, when coupled with the present preponderance of "mission-related" research support, could prove a serious detriment to the progress of science, by curtailing free research and by concentrating too much effort on trying to solve practical problems that currently appear insoluble. As Oppenheimer has pointed out regarding progress in research (*3*), "in the end you will be guided not by what it would be practically helpful to learn, but by what it is possible to learn."

Some idea of the relative magnitudes of national funds provided for "mission-related" and for "free" research, respectively, may be obtained as follows. Let us assume that all funds available in the following categories are provided for "mission-related" basic research: basic research by industry, by government laboratories, and by academic institutions from grants or contracts received from government agencies having practical missions. The latter chiefly include the Department of Defense, the Atomic Energy Commission, the Aeronautics and Space Administration, and the departments of Health, Education, and Welfare, Agriculture, Commerce, and Interior—agencies authorized and encouraged to conduct and support basic research related to their missions. On these assumptions, about 80 percent of the total national funds for basic research are provided for the "mission-related" variety, and only 20 percent for "free" basic research. These figures are far from precise, of course; the assumptions are oversimplified, and many agencies are liberal in their interpretation of what basic research may be useful to them. But the approximate magnitudes of the figures are significant, and illustrate my point.

Two observations concerning this are in order. First, if industry were to confine the research activities of its laboratories strictly to applied research and if the government were to place similar restrictions on agencies with practical missions, leaving the support of basic research entirely to a single federal agency, to private foundations, and to universities, it is reasonably certain that the support of basic research would drop to a mere fraction of its present figure. Second, while this might be attractive in budget circles, such a course would be disastrous not only to science but also to technology.

In raising the question as to the extent to which basic research is supported for essentially practical reasons, I wish to be entirely clear on one point. It is not my purpose to question the importance and desirability of applied research or of basic research which is intended to provide better insights into developmental applications. Both are highly desirable and necessary, and science should play a direct part in their encouragement. They appear to be logical steps in the march of civilization in that they represent progress in providing for necessities such as food, housing, health, communication, transportation, and the national defense. They also contribute attractive innovations in our way of life —comforts, pleasures, opportunities for using leisure, and freedom from routine and drudgery. Of longer-range significance are their effects upon control of our environment; upon extension, in magnitude and in kind, of our sources of commercial power; and upon the discovery and exploitation of natural resources. Above all, whether we like these developments or not, of one thing we may be certain: the forward march of technology is inevitable. This is an important lesson of history, from the discovery of fire and the invention of the wheel and the lever onward. It is a lesson which has withstood the ravages of heat and cold, famine and pestilence, and many ideological conflicts. The convincing proof of this doctrine is contained in science itself—the science of evolution—as the powerful contribution of technology toward survival, and indeed toward increasing domination over environment. During the present century, we are witnessing perhaps the greatest triumph of this doctrine in the conversion of all mankind to acceptance of the thesis that science and technology are essential to survival. This appears to be a thesis to which one must subscribe if one believes in the progress of civilization as we know it.

Historical Philosophical Role

But is this the whole story? From time immemorial man has evolved religions and philosophies representing his conviction that there is more to life than merely its physical aspects. Through imagination, study, and inspiration he has put forward philosophies, modes of conduct, and ways of life that concern the motivations and the aims of the individual and of society. From quite early times science has been thoroughly involved in much of this thinking, as evidenced by the earlier designation of natural science as "natural philosophy." From the record it is clear that, even after this term had fallen into disuse, science continued to have profound influence on philosophical thinking. It still does; witness the number of distinguished scientists of the present century who have written authoritative works on the subject—men such as Whitehead, Eddington, Jeans, Bridgman, and Dubos.

To what extent does this motivation for science still exist? How important is it? Are we observing, or failing to note, the gradual development of a monopoly by research oriented toward practical ends? There will of course always be individuals who firmly believe in the independence of research activity and who strongly wish to carry it on in the traditional academic manner. Will this group diminish in numbers or become frustrated? At the same time there appears to be a rapidly growing body of scientists employed in industry and government whose motivations are mixed, who believe in the support of basic research of the free variety but feel that "mission-related" basic research should have a higher priority, and still others who believe that research should be justified entirely on the basis of its specific utility.

Any uncertainty as to the importance of this question should be dispelled by looking into the history of science and noting: (i) the impressive discoveries made solely in the interest of pure science, and (ii) the statistical evidence that most of the body of science ultimately achieves practical utility.

Thus, even if we admit the requirement of utility as the prime justification for basic research, we still must allow free research to be included. It must be concluded that, in the long run, practical accomplishment will be greatest if in the support of basic research there is no limitation of the research to areas of foreseen practical importance.

I am reluctant to leave this topic without mentioning the thesis to which many, including myself, subscribe, to the effect that completely free research is highly important in its own right, not solely because of the probability that it will progress more rapidly and ultimately produce practical and tangible benefits, but because of its stimulating effect on the imagination and its philosophical implications concerning the universe and man's place in it. Who can say that ultimately this may not be the most important consideration of all?

Source of Strength

I wish now to consider a different but related feature of my topic: What is the secret of the power and influence of science in the most fundamental sense? Is its source of strength at all in jeopardy? If so, are there any steps we ought to take to safeguard its future?

This body of knowledge—science in the modern sense—has steadily developed over the past 400-odd years into a most imposing edifice. Once science had discovered the art of experimentation it found a way to test hypotheses and speculation regarding the nature and behavior of the physical world and thus established a powerful base for drawing objective conclusions. This, together with the development, along with mathematics and logic, of techniques of classification and analysis, united the findings of science into a structure of extraordinary strength and stability. Furthermore, this technique has had a highly democratic flavor: anyone can challenge the alleged facts and theories of science. If he can prove his point within the scientific community by observations, experiments, or reasoning that others can repeat and verify, then his contribution becomes an integral part of the body of science. Science has thus acquired a respect and confidence on the part of literate mankind that is unique. In consequence, the findings of science have a logical validity which is unmatched in other fields of human thought. At the same time, in a most interesting manner science remains flexible, since important new findings may necessitate revision of existing points of view. Generally speaking, and contrary to popular view, these revisions commonly take the form of refinements or increased generality and only occasionally bring about a revolutionary overthrow of existing principles. The impressive result is that the edifice of science has a strength and stability which is dynamic and resilient rather than static and brittle.

How do we account for these characteristics? They appear to be due to the maintenance of a broad base of inquiry; to the exercise of a lively imagination; to the utmost objectivity in search and logic; to a sense of proportion and urgency in the selection of scientific objectives. One must also recognize the necessity of built-in mechanisms for coordination, cross fertilization, and collaboration, and finally—most important of all—of a creative dedication. These are high ideals, not commonly encountered or possible to the same degree in most other areas of human affairs and requiring a high degree of motivation and integrity.

These principles and this code of behavior are thoroughly learned by every researcher, beginning with his years of graduate study. It has been a source of the greatest strength to the body of science that, on the whole, these principles have been scrupulously observed. There has been no means of enforcement other than public opinion within the scientific community. Just as the standing of an individual in his field of research rests primarily with his colleagues, so too does his reputation in his behavior as a scientist. The real strength of this philosophy lies in the fact that these principles are essential for sound progress in science.

Thus, much of the power and stability of science has rested upon the sense of dedication and integrity of the community of scientists. Not only has this been thoroughly incorporated in their indoctrination but it has been further developed and fostered as a code of honor among scientists: to be scrupulously objective in their research, in their reporting, and in giving credit where credit is due. It would seem that the chief reason for the almost universal observance of this code has been that the scientist desires the respect and confidence of his colleagues, rather than recognition before any other audience. Anyone departing from these rules of behavior is ostracized by his kind.

Encroachment of Other Loyalties

In most careers, however, loyalties and motivations are more complicated. They involve such considerations as allegiance to, and recognition by, one's employer and his organization, one's community, church, political party, and friends, and the public generally. An interesting question is the extent to which these other loyalties are increasing in importance among scientists and encroaching upon loyalties to the scientific community. If so, will this warp or weaken the edifice of science or retard its progress?

Profit institutions such as industrial laboratories are, of course, clear examples of organizations that require strong loyalties in carrying out purposes related to the well-being of the organization. The same may be said of government establishments. Because of the increasing proportion, in the scientific community, of scientists employed by industry and government, these considerations are inevitably coming to receive more and more emphasis.

Likewise, with increasing dependence of colleges and universities upon the federal government, federal support of scientific research at these institutions becomes more and more strongly related to their health and strength. Again, this may be manifested in tangible or intangible pressures on the part of academic institutions for their scientists to engage in sponsored activities which are deemed essential to the growth and welfare of the institution and which may bring with them the necessary financing.

Also, in the "project" type of support, members of the scientific community are becoming directly and increasingly motivated toward engaging in research which is regarded as important by a sponsoring agency of the government rather than by their employer. Since most federal support is directed toward practical goals which will serve the needs of the country, there are incentives for an individual to engage in research which will receive this support and therefore may come under the heading of "mission-related" rather than "free" research.

Let me say again that research motivated toward practical ends is a necessary and desirable thing; the potential danger here is the extent to which this objective dominates the scope and purpose of basic research. It was succinctly formulated by Vannevar Bush when he remarked that applied research drives out basic, and I am now using the statement to include also the possible encroachment of "mission-related" basic research upon the "free" variety.

By the way, what will happen if the ceiling on R&D funds is held more and more tightly? If we believe in substantial support for free research, with its admittedly vague and uncertain potentialities, how are we going to protect it? Will it have to depend upon income from capital funds—if so, from what sources? Or will its advocates try to oversell it by extravagant claims?

The influences that govern scientists in their choice of research and their choice of employment are more complex than ever before. Today's ivory tower is more apt to be built of reinforced concrete or stainless steel. These influences are many, some major and some detailed. For example, in addition to the competition between applied and basic research, there are considerations such as needy areas, attractive sources of funds, national or humanitarian causes, "big" versus "little" science, and deference to the plans of one's department or institution. A different kind of influence on research is represented by the following: too much assistance to thesis-writing graduate students, with an eye toward grant or contract renewal; hasty writing and issuance of research reports, scanty in detail and acknowledgment; a tendency to keep a weather eye on funds for extra salary or other perquisites. Further complications are provided by administrative requirements which seem essential to management in large organizations as a means of accounting for public funds, but which distract and hamper the researcher.

But I do not wish to sound too pessimistic. As a matter of fact, I have had rather extensive contact over the past years with scientists in senior academic administrative posts and can assure you that, by and large, they understand these problems and try to hold them within manageable limits. The real danger lies in the fact that in such an extensive enterprise there are bound to be abuses. If these are not dealt with forthrightly they may spontaneously proliferate until there is clamor for formal corrective regulation.

If one were to classify the sources of influence, the first and obvious category would be money—money for projects, buildings, research equipment, salaries, and many minor perquisites. A second category would be the employing institution, in its desire for income, growth, and prestige. One would also have to list the increasing effect of personal advancement or gain associated with the positions of high responsibility, salary, and prestige which are now available to scientists.

Even science itself is providing dilemmas for an individual scientist. Should he join an interdisciplinary team in which his specialty is needed, join a large research center such as a high-energy particle accelerator installation, take part in an extensive planned program, such as oceanography or the study of pollution? Or should he remain aloof as an individual investigator? And what about his responsibility toward teaching?

Conclusion

Of course the consequence of all this may be the broadening out of a scientific career into one more closely integrated with society in general. This is natural enough, and surely after careful consideration most would agree that this result is desirable. My question today directly concerns the necessity for maintaining the strength and integrity of science in the face of varied opportunities, responsibilities, and distractions: How should this strength and integrity be safeguarded? If the involvement of scientists in social affairs brings with it questionable or dangerous consequences to society, then society will take steps to formulate regulations for their prevention, with possible grave effect upon science. Similarly, in science itself, if the course of science and the behavior of scientists appear to scientists themselves to be damaging to its strength and progress, then a normal reaction on their part would be the formulation of rules and regulations to prevent such abuses.

However, in order to maintain and protect the independence and creative quality of basic research in science, one should, I believe, conclude that such modes of regulation should only be attempted as a last resort, and even then as sparingly as possible. It should be clear that the most effective means of maintaining the objectives and initiative that have always characterized science is still the cultivation and retention of a strong sense of competition, cooperation, and integrity on the part of all scientists. All we need do is to continue and strengthen our time-honored traditions. But this is not going to be easy. We shall have to distinguish clearly between our conduct in our science and our behavior in the presence of issues that go beyond science alone. Judgment and objectivity are still required on such issues; the main differences are that these decisions, in contrast to science, require the weighing of opinions and pressures, as well as facts, and the attempt to make value judgments between items that are not comparable. Moreover, in the world of science, compromise has no place; in the world of affairs it must often be reckoned with, and occasionally sought.

I cannot close without mentioning a great opportunity before us which may and should become a most effective avenue for the healthy growth and influence of science. I refer to the progress made in international science programs. As is well known, science has always transcended national boundaries, and scientists of all nations have communicated and collaborated in all its disciplines. There are two categories of research for which international collaboration is especially well suited. The one includes matters of urgent public concern, and is typified by the World Health Organization and the World Meteorological Organization. Of the nature of applied research and development, these matters are, appropriately, planned and sponsored by formal agreement among governments under UNESCO. Such problems as population control, insurance against war, famine, drought and pestilence, and the development of natural resources belong in this category. In all these, science can provide a unique input, the effectiveness of which will depend directly upon the recognition of this fact by governments and people everywhere, and upon intelligent and widespread support by them.

The other category is research concerned with fields of basic research, such as geophysics and astronomy, which require concerted global observation and collection of data. Frequently this is an interesting combination of "mission-related" and "free" research. The International Council of Scientific Unions is performing meritorious service in providing a focus for these endeavors. The outstanding example, of course, is the International Geophysical Year (IGY) and its offspring—the Indian Ocean Expedition, the International Year of the Quiet Sun, the Earth Mantle Project, and the International Biological Year. Unique among these is the Antarctic Research Program, where the IGY program is continued under a 12-nation treaty, expressly and solely for purposes of scientific research.

It is in such areas that scientists are eminently qualified to plan and to operate, and it is in the highest interests of both science and government that they do so. Plans thus formulated may be submitted to their respective governments for support and any formal arrangements needed.

But, beyond this, we stand at the threshold of scientific findings that will pave the way for developments of a different order of magnitude and novelty than the world has ever known. A few are already in sight—notably the exploration of space; others are as yet beyond the horizon. Some will present severe social problems; some may be dangerous; some will be extremely expensive. All will present questions for society that go far beyond the natural sciences alone; they will strongly involve the social sciences and the humanities. They will provide inspiration for the arts. To solve these problems will require many of the skills of our civilization, the utmost in statesmanship, and a general understanding and appreciation on the part of all.

The significance of these developing enterprises in science and technology, their hazards, and their excessive cost in money and manpower point to the overwhelming desirability of international cooperation. Herein lies our great opportunity as scientists—to take the lead in collaboration with our colleagues in other lands and to support our governments in furthering such collaboration.

It would be a tragedy indeed if these undertakings were to become the subject of national or sectional ambition under conditions of unfriendly competition. On the other hand, if we can help achieve an atmosphere of collaboration, in friendly competition, we may look forward to continued healthy progress in our ideals and in our accomplishments for the future of mankind.

References and Notes

1. See *Science, Economic Growth and Government Policy* (Organization for Economic Co-operation and Development, 1963).
2. Canada's increase was once interrupted by a sudden change in provision for industrial development of military aircraft.
3. Exceptions are Japan and the Netherlands, where the manufacturing industry is well developed and there are no large defense, nuclear, or space programs.
4. J. R. Oppenheimer, *The Flying Trapeze: Three Crises for Physicists* (Oxford Univ. Press, New York, 1964).

CRITERIA FOR SCIENTIFIC CHOICE

By Alvin M. Weinberg

As science grows, its demands on our society's resources grow. It seems inevitable that science's demands will eventually be limited by what society can allocate to it. We shall then have to make choices. These choices are of two kinds. We shall have to choose among different, often incommensurable, fields of science—between, for example, high-energy physics and oceanography or between molecular biology and science of metals. We shall also have to choose among the different institutions that receive support for science from the government—among universities, governmental laboratories, and industry. The first choice I call scientific choice; the second, institutional choice. My purpose is to suggest criteria for making scientific choices—to formulate a scale of values which might help establish priorities among scientific fields whose only common characteristic is that they all derive support from the government.

Choices of this sort are made at every level, both in science and in government. The individual scientist must decide what science to do, what not to do: the totality of such judgments makes up his scientific taste. The research director must choose which projects to push, which to kill. The government administrator must decide not only which efforts to support; he must also decide whether to do a piece of work in a university, a national laboratory, or an industrial laboratory. The sum of such separate decisions determines our policy as a whole. I shall be concerned mainly with the broadest scientific choices: how should government decide between very large fields of science, particularly between different branches of basic science? The equally important question of how government should allocate its support for basic research among industry, governmental laboratories, and universities will not be discussed here.

Most of us like to be loved; we hate to make choices, since a real choice alienates the party that loses. If one is rich—more accurately, if one is growing richer—choices can be avoided. Every administrator knows that his job is obviously unpleasant only when his budget has been cut. Thus the urgency for making scientific or institutional choices has in the main been ignored both in the United States and elsewhere because the science budget has been expanding so rapidly: the United States government spent $1.6 billion in 1950 on research and development, $9 billion in 1960, and $14 billion (including space) in 1962.

Though almost all agree that choices will eventually have to be made, some well-informed observers insist that the time for making the choices is far in the future. Their arguments against making explicit choices have several main threads. Perhaps most central is the argument that since we do not make explicit choices about anything else, there is no reason why we should make them in science. Since we do not explicitly choose between support for farm prices and support for schools, or between highways and foreign aid, why should we single out science as the guinea pig for trying to make choices? The total public activity of our society has always resulted from countervailing pressures, exerted by various groups representing professional specialties, or local interests, or concern for the public interest. The combination that emerges as our federal budget is not arrived at by the systematic application of a set of criteria; even the highest level of authority, in the United States, the President, who must weigh conflicting interests in the scale of the public interest, is limited in the degree to which he can impose an over-all judgment by the sheer size of the budget if by nothing else. But because we have always arrived at an allocation by the free play of countervailing pressures this does not mean that such free interplay is the best or the only way to make choices. In any case, even if our choices remain largely implicit rather than explicit, they will be more reasonable if persons at every level, representing every pressure group, try to understand the larger issues and try to mitigate sectional self-interest with concern for broader issues. The idea of conflicting and biased claims being adjudicated at one fell swoop by an all-knowing supreme tribunal is a myth. It is much better that the choices be decentralized and that they reflect the concern for the larger interest. For this reason alone philosophic debate on the problems of scientific choice should lead to a more rational allocation of our resources.

A second thread in the argument of those who refuse to face the problem of scientific choice is that we waste so much on trivialities—on smoking,

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on advertising, on gambling—that it is silly to worry about expenditures of the same scale on what is obviously a more useful social objective, the increase of scientific knowledge. A variant of this argument is that with so much unused steel capacity or so many unemployed, we cannot rightly argue that we cannot afford a big cyclotron or a large manned space venture.

Against these arguments we would present the following considerations on behalf of a rational scientific policy. At any given instant, only a certain fraction of our society's resources goes to science. To insist or imply that the summum bonum of our society is the pursuit of science and that therefore all other activities of the society are secondary to science—that unused capacity in the steel mills should go to "Big Science" rather than a large-scale housing program—is a view that might appeal strongly to the scientific community. It is hardly likely to appeal so strongly to the much larger part of society that elects the members of the legislature, and to whom, in all probability, good houses are more important than good science. Thus, as a practical matter we cannot really evade the problem of scientific choice. If those actively engaged in science do not make choices, they will be made anyhow by the Congressional Appropriations Committees and by the Bureau of the Budget, or corresponding bodies in other governments. Moreover, and perhaps more immediately, even if we are not limited by money, we shall be limited by the availability of truly competent men. There is some evidence that our ratio of money to men in science is too high, and that in some parts of science we have gone further more quickly than the number of really competent men can justify.

Choice and scientific criticism

Our scientific and governmental communities have evolved institutional and other devices for coping with broad issues of scientific choice. The most important institutional device in the United States is the President's Science Advisory Committee, with its panels and its staff in the Office of Science and Technology. This body and its panels help the Bureau of the Budget to decide what is to be supported and what is not to be supported. The panel system, however, suffers from a serious weakness. Panels usually consist of specialized experts who inevitably share the same enthusiasms and passions. To the expert in oceanography or in high-energy physics, nothing seems quite as important as oceanography or high-energy physics. The panel, when recommending a program in a field in which all its members are interested, invariably argues for better treatment of the field—more money, more people, more training. The panel system is weak insofar as judge, jury, plaintiff, and defendant are usually one and the same.

The panel is able to judge how competently a proposed piece of research is likely to be carried out; its members are all experts and are likely to know who are the good research workers in the field. But just because the panel is composed of experts, who hold parochial viewpoints, the panel is much less able to place the proposal in a broader perspective and to say whether the research proposal is of much interest to the rest of science. We can answer the question "how" within a given frame of reference; it is impossible to answer "why" within the same frame of reference. It would therefore seem that the panel system could be improved if representatives, not only of the field being judged but also representatives of neighboring fields, sat on every panel judging the merits of a research proposal. A panel judging high-energy physics should have some people from low-energy physics; a panel judging low-energy physics should have some people from nuclear energy; a panel judging nuclear energy should have some people from conventional energy; and so on. I should think that advice from panels so constituted would be tempered by concern for larger issues; in particular, the support of a proposed research project would be viewed from the larger perspective of the relevance of that research to the rest of science.

In addition to panels or the bodies like the President's Science Advisory Committee as organizational instruments for making choices, the scientific community has evolved an empirical method for establishing scientific priorities, that is, for deciding what is important in science and what is not important. This is the scientific literature. The process of self-criticism, which is integral to the literature of science, is one of the most characteristic features of science. Nonsense is weeded out and held up to ridicule in the literature, whereas what is worthwhile receives much sympathetic attention. This process of self-criticism embodied in the literature, though implicit, is nonetheless real and highly significant. The existence of a healthy, viable scientific literature in itself helps assure society that the science it supports is valid and deserving of support. This is a most important, though little recognized, social function of the scientific literature.

As an arbiter of scientific taste and validity, scientific literature is beset with two difficulties.

First, because of the information explosion, the literature is not read nearly as carefully as it used to be. Nonsense is not so generally recognized as such, and the standards of self-criticism, which are so necessary if the scientific literature is to serve as the arbiter of scientific taste, are inevitably looser than they once were.

Second, the scientific literature in a given field tends to form a closed universe; workers in a field, when they criticize each other, tend to adopt the same unstated assumptions. A referee of a scientific paper asks whether the paper conforms to the rules of the scientific community to which both referee and author belong, not whether the rules themselves are valid. So to speak, the editors and authors of a journal in a narrowly specialized field are all tainted with the same poison.

Can a true art of scientific criticism be developed, i.e., can one properly criticize a field of science beyond the kind of criticism that is inherent in the literature of the field? Mortimer Taube in *Computers and Common Sense*[2] insists that such scientific criticism is a useful undertaking, and that, by viewing a field from a somewhat detached point of view, it is possible to criticize a field meaningfully, even to the point of calling the whole activity fraudulent, as he does in the case of nonnumerical uses of computers. I happen to believe that Taube does not make a convincing case in respect to certain nonnumerical uses of computers, such as language translation. Yet I have sympathy for Dr. Taube's aims—that, with science taking so much of the public's money, we must countenance, even encourage, discussion of the relative validity and worthwhileness of the science which society supports.

The internal criteria for choice

I believe that criteria for scientific choice can be identified. In fact, several such criteria already exist; the main task is to make them more explicit. The criteria can be divided into two kinds: internal criteria and external criteria. Internal criteria are generated within the scientific field itself and answer the question: How well is the science done? External criteria are generated outside the scientific field and answer the question: Why pursue this particular science? Though both are important, I think the external criteria are the more important.

Two internal criteria can be easily identified: (1) Is the field ready for exploitation? (2) Are the scientists in the field really competent? Both these questions are answerable only by experts who know the field in question intimately, and who know the people personally. These criteria are therefore the ones most often applied when a panel decides on a research grant; in fact, the primary question in deciding whether to provide governmental support for a scientist is usually: How good is he?

I believe, however, that it is not tenable to base our judgments entirely on internal criteria. As I have said, we scientists like to believe that the pursuit of science as such is society's highest good, but this view cannot be taken for granted. For example, we now suffer a serious shortage of medical practitioners, probably to some extent because many bright young men who would formerly have gone into medical practice now go into biological research; government support is generally available for postgraduate study leading to the PhD but not for study leading to the medical degree. It is by no means self-evident that society gains from more biological research and less medical practice. Society does not a priori owe the scientist, even the good scientist, support any more than it owes the artist or the writer or the musician support. Science must seek its support from society on grounds other than that the science is carried out competently and that it is ready for exploitation; scientists cannot expect society to support science because scientists find it an enchanting diversion. Thus, in seeking justification for the support of science, we are led inevitably to consider external criteria for the validity of science—criteria external to science, or to a given field of science.

External criteria

Three external criteria can be recognized: technological merit, scientific merit, and social merit. The first is fairly obvious: once we have decided, one way or another, that a certain technological end is worthwhile, we must support the scientific research necessary to achieve that end. Thus, if we have set out to learn how to make breeder reactors, we must first measure painstakingly the neutron yields of the fissile isotopes as a function of energy of the bombarding neutron. As in all such questions of choice, it is not always so easy to decide the technological relevance of a piece of basic research. The technological usefulness of the laser came after, not before, the principle of optical amplification was discovered, and, in general, indirect technological or scientific benefits ("fallout") are not uncommon. But it is my belief that such technological bolts from the scientific blue are the exception, not the rule, that solving a technological problem by waiting for fallout from an entirely different field is rather overrated. Most programmatic basic research can be related fairly directly to a technological end, at least crudely if not in detail.

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[2] (New York: Columbia University Press, 1961).

The broader question as to whether the technological aim itself is worthwhile must be considered again partly from within technology through answering such questions as: Is the technology ripe for exploitation? Are the people any good? It must also be dealt with partly from outside technology by answering the question: Are the social goals attained, if the technology succeeds, themselves worthwhile? Many times these questions are difficult to answer, and sometimes they are answered incorrectly; for example, the United States launched an effort to control thermonuclear energy in 1952 on a rather large scale because it was thought at the time that controlled fusion was much closer at hand than it turned out to be. Nevertheless, despite the fact that we make mistakes, technological aims are customarily scrutinized much more closely than are scientific aims; at least we have more practice discussing technological merit than we do scientific merit.

The criteria of scientific merit and social merit are much more difficult: scientific merit because we have given little thought to defining scientific merit in the broadest sense; social merit because it is difficult to define the values of our society. As I have already suggested, the answer to the question: Does this broad field of research have scientific merit? cannot be answered within the field. The idea that the scientific merit of a field can be judged better from the vantage point of the scientific fields in which it is embedded than from the point of view of the field itself is implicit in the following quotation from the late John von Neumann: "As a mathematical discipline travels far from its empirical source, or still more, if it is a second and third generation only indirectly inspired by ideas coming from reality, it is beset with very grave dangers. It becomes more and more pure aestheticizing, more and more purely l'art pour l'art. This need not be bad if the field is surrounded by correlated subjects which still have closer empirical connections or if the discipline is under the influence of men with an exceptionally well-developed taste. But there is a grave danger that the subject will develop along the line of least resistance, that the stream, so far from its source, will separate into a multitude of insignificant branches, and that the discipline will become a disorganized mass of details and complexities. In other words, at a great distance from its empirical source, or after much 'abstract' inbreeding, a mathematical subject is in danger of degeneration. At the inception the style is usually classical; when it shows signs of becoming baroque, then the danger signal is up."[8]

I believe there are any number of examples to show that von Neumann's observation about mathematics can be extended to the empirical sciences. Empirical basic sciences which move too far from the neighboring sciences in which they are embedded tend to become "baroque." Relevance to neighboring fields of science is, therefore, a valid measure of the scientific merit of a field of basic science. In so far as our aim is to increase our grasp and understanding of the universe, we must recognize that some areas of basic science do more to round out the whole picture than do others. A field in which lack of knowledge is a bottleneck to the understanding of other fields deserves more support than a field which is isolated from other fields. This is only another way of saying that, ideally, science is a unified structure and that scientists, in adding to the structure ought always to strengthen its unity. Thus, the original motivation for much of high-energy physics is to be sought in its elucidation of low-energy physics, or the strongest and most exciting motivation for measuring the neutron capture cross sections of the elements lies in the elucidation of the cosmic origin of the elements. Moreover, the discoveries which are acknowledged to be the most important scientifically, have the quality of bearing strongly on the scientific disciplines around them. For example, the discovery of x rays was important partly because it extended the electromagnetic spectrum but, much more, because it enabled us to see so much that we had been unable to see. The word "fundamental" in basic science, which is often used as a synonym for "important," can be partly paraphrased into "relevance to neighboring areas of science." I would therefore sharpen the criterion of scientific merit by proposing that, other things being equal, *that field has the most scientific merit which contributes most heavily to and illuminates most brightly its neighboring scientific disciplines.* This is the justification for my previous suggestion about making it socially acceptable for people in *related* fields to offer opinions on the scientific merit of work in a given field. In a sense, what I am trying to do is to extend to basic research a practice that is customary in applied science: a project director trying to get a reactor built on time is expected to judge the usefulness of component development and fundamental research which bear on his problems. He is not always right; but his opinions are usually useful both to the researcher and to the management disbursing the money.

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[8] Heywood, R. B. (ed.), *The Works of the Mind* (University of Chicago Press, 1947), p. 196.

I turn now to the most controversial criterion of all—social merit or relevance to human welfare and the values of man. Two difficulties face us when we try to clarify the criterion of social merit: first, who is to define the values of man, or even the values of our own society; and second, just as we shall have difficulty deciding whether a proposed research helps other branches of science or technology, so we will have even greater trouble deciding whether a given scientific or technical enterprise indeed furthers our pursuit of social values, even when those values have been identified. With some values we have little trouble: adequate defense, or more food, or less sickness, for example, are rather uncontroversial. Moreover, since such values themselves are relatively easy to describe, we can often guess whether a scientific activity is likely to be relevant, if not actually helpful, in achieving the goal. On the other hand, some social values are much harder to define: perhaps the most difficult is national prestige. How do we measure national prestige? What is meant when we say that a man on the moon enhances our national prestige? Does it enhance our prestige more than, say, discovering a polio vaccine or winning more Nobel Prizes than any other country? Whether or not a given achievement confers prestige probably depends as much on the publicity that accompanies the achievement as it does on its intrinsic value.

Among the most attractive social values that science can help to achieve is international understanding and cooperation. It is a commonplace that the standards and loyalties of science are transnational. A new element has recently been injected by the advent of scientific research of such costliness that now it is prudent as well as efficient to participate in some form of international cooperation. The very big accelerators are so expensive that international laboratories such as CERN at Geneva are set up to enable several countries to share costs that are too heavy for them to bear separately. Even if we were not committed to improving international relations we would be impelled to cooperate merely to save money.

Bigness is an advantage rather than a disadvantage if science is to be used as an instrument of international cooperation; a $500 million cooperative scientific venture—such as the proposed 1000 BeV intercontinental accelerator—is likely to have more impact than a $500 000 Van de Graaff machine. The most expensive of all scientific or quasi-scientific enterprises—the exploration of space—is, from this viewpoint, the best-suited instrument for international cooperation. The exchange between President Kennedy and Chairman Khrushchev concerning possible increased cooperation in space exploration seems to have been well received and, one hopes, will bear ultimate fruit.

Some specific fields assessed

Having set forth these criteria and recognizing that judgments are fraught with difficulty, I propose, in their light, to assess five different scientific and technical fields: molecular biology, high-energy physics, nuclear energy, manned-space exploration, and the behavioral sciences. Two of these fields, molecular biology and high-energy physics, are, by any definition, basic sciences; nuclear energy is applied science, the behavioral sciences are a mixture of both applied and basic science. Manned exploration of space, though it requires the tools of science and is regarded in the popular mind as being part of science, has not yet been proved to be more than quasi-scientific, at best. The fields which I choose are incommensurable: how can one measure the merit of behavioral sciences and nuclear energy on the same scale of values? Yet the choices between scientific fields will eventually have to be made whether we like it or not. Criteria for scientific choice will be most useful only if they *can* be applied to seemingly incommensurable situations. The validity of my proposed criteria depends on how well they can serve in comparing fields that are hard to compare.

Of the scientific fields now receiving public support, perhaps the most successful is molecular biology. Hardly a month goes by without a stunning success in molecular biology being reported in the *Proceedings of the National Academy of Sciences.* The most recent has been the cracking by Nierenberg and Ochoa of the code according to which triples of bases determine specific amino acids in the living proteins. Here is a field which rates the highest grades as to its ripeness for exploitation and competence of its workers. It is profoundly important for large stretches of other biological sciences—genetics, cytology, microbiology—and, therefore, according to my criterion, must be graded A+ for its scientific merit. It also must be given a very high grade in social merit, and probably in technological (that is, medical) merit

—more than, say, taxonomy or topology. Molecular biology is the most fundamental of all the biological sciences. With understanding of the manner of transmission of genetic information ought to come the insights necessary for the solution of such problems as cancer, birth defects, and viral diseases. Altogether, molecular biology ought, in my opinion, to receive as much public support as can possibly be pumped into it; since money is not limiting its growth, many more post-graduate students and research fellows in molecular biology ought to be subsidized so that the attack on this frontier can be expanded as rapidly as possible.

The second field is high-energy physics. This field of endeavor originally sought as its major task to understand the nuclear force. In this it has been only modestly successful; instead, it has opened an undreamed-of subnuclear world of strange particles, a world in which mirror images are often reversed. The field has no end of interesting things to do, it knows how to do them, and its people are the best. Yet I would be bold enough to argue that, at least by the criteria which I have set forth—relevance to the sciences in which it is embedded, relevance to human affairs, and relevance to technology—high-energy physics rates poorly. The world of subnuclear particles seems to be remote from the rest of the physical sciences. Aside from the brilliant resolution of the τ-particle paradox, which led to the overthrow of the conservation of parity, and the studies of mesic atoms (the latter of which is not done at *ultra*-high energy), I know of few discoveries in ultra-high-energy physics which bear strongly on the rest of science. This view must be tempered by the fairly considerable indirect fallout from high-energy physics—for example, the use of strong focusing, the development of ultra-fast electronics, and the possibility of using machines like the Argonne ZGS as very strong, pulsed sources of neutrons for study of neutron cross sections. As for its direct bearing on human welfare and on technology, I believe it is essentially nil. These two low grades would not bother me if high-energy physics were cheap. But it is terribly expensive—not so much in money as in highly qualified people, especially those brilliant talents who could contribute so ably to other fields which contribute much more to the rest of science and to humanity than does high-energy physics. On the other hand, if high-energy physics could be strengthened as a vehicle for international cooperation—if the much-discussed intercontinental 1000 BeV accelerator could indeed be built as a joint enterprise between East and West—the expense of high-energy physics would become a virtue, and the enterprise would receive a higher grade in social merit than I would now be willing to assign to it.

Third is nuclear energy, a field toward which I have passion and aspiration, and therefore am not unbiased. Being largely an applied effort, nuclear energy is very relevant to human welfare. We now realize that in the residual uranium and thorium of the earth's crust, mankind has an unlimited store of energy—enough to last for millions of years; and that with an effort of only one-tenth of our manned space effort we could, within ten or fifteen years, develop the reactors which would tap this resource. Only rarely do we see ways of *permanently* satisfying one of man's major needs—in this case energy. In high-conversion-ratio nuclear reactors we have such means, and we are close to their achievement. Moreover, we begin to see ways of applying very large reactors of this type to realize another great end, the economic desalination of the ocean. Thus, the time is very ripe for exploitation. Nuclear energy rates so highly in the categories of technical and social merit and timeliness that I believe it deserves strong support, even if it gets very low marks in the other two categories—its personnel and its relationship to the rest of science. Suffice it to say that in my opinion the scientific workers in the field of nuclear energy are good and that nuclear energy in its basic aspects has vast ramifications in other scientific fields.

Next on the list are the behavioral sciences—psychology, sociology, anthropology, and economics. The workers are of high quality; the sciences are significantly related to each other, they are deeply germane to every aspect of human existence. In these respects the sciences deserve strong public support. On the other hand, it is not clear to me that the behavioral scientists, on the whole, see clearly how to attack the important problems of their sciences. Fortunately, the total sum involved in behavioral science research is now relatively tiny—as it well must be when what are lacking are deeply fruitful, and generally accepted, points of departure.

Finally, I come to manned space exploration. The personnel in the program are competent and dedicated. With respect to ripeness for exploitation, the situation seems to me somewhat unclear. Our "hardware" is in good shape, and we can expect it to get better—bigger and more reliable boosters, better communication systems, etc. What is not clear is the human being's tolerance of the space environment. I do not believe that either the hazards of radiation or of weightlessness are sufficiently explored yet positively to guarantee success in our future manned space ventures.

The main objection to spending so much manpower, as well as money, on manned space exploration is its remoteness from human affairs, not to say the rest of science. In this respect, space (the exploration of very large distances) and high-energy physics (the exploration of very small distances) are similar, though high-energy physics has the advantage of greater scientific validity. There are some who argue that the great adventure of man into space is not to be judged as science, but rather as a quasi-scientific enterprise, justified on the same grounds as those on which we justify other nonscientific national efforts. The weakness of this argument is that space requires many, many scientists and engineers, and these are badly needed for such matters as clarifying our civilian defense posture or, for that matter, working out the technical details of arms control and foreign aid. If space is ruled to be nonscientific, then it must be balanced against other nonscientific expenditures like highways, schools, or civil defense. If we do space research because of prestige, then we should ask whether we get more prestige from a man on the moon than from successful control of the waterlogging problem in Pakistan's Indus Valley Basin. If we do space research because of its military implications, we ought to say so—and perhaps the military justification, at least for developing big boosters, is plausible, as the Soviet experience with rockets makes clear.

The big problem of "Big Science"

The main weight of my argument is that the most valid criteria for assessing scientific fields come from without rather than from within the scientific discipline that is being rated. This does not mean that only those scientific fields deserve priority that have high technical merit or high social merit. Scientific merit is as important as the other two criteria, but, as I have argued, scientific merit must be judged from the vantage point of the scientific fields in which each field is embedded rather than from that of the field itself. If we support science in order to maximize our knowledge of the world around us, then we must give the highest priority to those scientific endeavors that have the most bearing on the rest of science.

The rather extreme view which I have taken presents difficulties in practice. The main trouble is that the bearing that one science has on another science so often is not appreciated until long after the original discoveries have been made. Who was wise enough, at the time Purcell and Bloch first discovered nuclear magnetic resonance, to guess that the method would become an important tool for unravelling chemical structures? Or how could one have guessed that Hahn and Strassmann's radiochemical studies would have led to nuclear energy? And indeed, my colleagues in high-energy physics predict that what we learn about the world of strange particles will in an as yet undiscernible way teach us much about the rest of physics, not merely much about strange particles. They beg only for time to prove their point.

To this argument I say first that choices are always hard. It would be far simpler if the problem of scientific choice could be ignored, and possibly in some future millennium it can be. But there is also a more constructive response. The necessity for scientific choice arises in "Big Science," not in "Little Science". Just as our society supports artists and musicians on a small scale, so I have no objection to—in fact, I strongly favor—our society supporting science that rates zero on all the external criteria, provided it rates well on the internal criteria (ripeness and competence) and provided it is carried out on a relatively small scale. It is only when science does make serious demands on the resources of our society—when it becomes "Big Science"—that the question of choice really arises.

At the present time, with our society faced with so much unfinished and very pressing business, science can hardly be considered its major business. For scientists as a class to imply that science can, at this stage in human development, be made the main business of humanity is irresponsible—and, from the scientist's point of view, highly dangerous. It is quite conceivable that our society will tire of devoting so much of its wealth to science, especially if the implied promises held out when big projects are launched do not materialize in anything very useful. I shudder to think what would happen to science in general if our manned-space venture turned out to be a major failure, if it turned out, for example, that man could not withstand the re-entry deceleration forces after a long sojourn in space. It is as much out of a prudent concern for their own survival, as for any loftier motive, that scientists must acquire the habit of scrutinizing what they do from a broader point of view than has been their custom. To do less could cause a popular reaction which would greatly damage mankind's most remarkable intellectual attainment—modern science—and the scientists who created it and must carry it forward.

CRITERIA FOR SCIENTIFIC CHOICE II: THE TWO CULTURES

ALVIN M. WEINBERG

The Financial Support of " Science as a Whole "

IN a previous paper [1] I proposed criteria which could be invoked in judging how to allocate support to different, competing branches of basic science. Such allocations seem to be necessary because what society is willing to spend on all of science is not enough to satisfy every worthy claim on the total funds available for science. I turn now to the broader question: what criteria can society use in deciding how much it can allocate to science as a whole rather than to competing activities such as education, social security, foreign aid and the like?

That such a question can assume any urgency is in itself remarkable. To have suggested that the Federal Government of the United States would be spending about 3 per cent. of the gross national product for research and development would have been unbelievable 25 years ago. Most of the new attitude toward government support of science and technology was prompted by war and fear of war. In the mind of the public, scientific strength has been equated with military strength. Support of science at first was only dimly distinguished from support of the military. But this attitude is changing, partly because the thermonuclear stalemate seems to have reduced our fear of war, partly because the fantastic successes of modern science have begun to penetrate the awareness of the public. Science *per se*, as a valid human activity supported by the public, has acquired some standing, possibly analogous to that of religion in the era before the separation of church and state. As science has become big, it has acquired imperatives, just like any other activity of government, to expand and to demand an increasing share of public resources, and now, for the first time, it has become big enough to compete seriously for money with other major activities of government.

The criteria for choice between different fields of basic science I proposed earlier were of two kinds—internal and external. Internal criteria could be established entirely within the scientific field being considered; these criteria arise from the question: how competently is this field of science performed? External criteria could be established only from outside and answered the questions: does this field of science illuminate other fields of science; does it further desirable technological goals; does it further broad social goals? My main point was that a good rating according to the internal criteria was a necessary but not sufficient condition for large-scale public support of a field of science. Only if a field rated highly according to criteria generated outside its own universe could it properly expect large-scale support by society.

In so far as the support of science as a whole can be viewed as different from support of each of the separate branches and kinds of science, I believe one can apply analogous criteria. Society, in its support of science, assumes that science is a competent, responsible undertaking. But society is justified in asking more than this of "science as a whole". However vaguely stated, society expects science somehow to serve certain social goals outside science itself. It applies criteria from without science—broadly, criteria concerned with human values—when it assesses the proper role of "science as a whole" relative to other activities. We scientists concede this implicitly when we agree that responsibility for choosing between science and other activities belongs primarily to the non-scientists—the members of legislative bodies or the head of the executive branch of government and his staff. In the language of Stephen Toulmin[2] the choice between "science done for its own sake" and other activities of the society is a political choice, as contrasted to an administrative choice, and it is to be made by politicians.

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[1] " Criteria for Scientific Choice ", *Minerva*, I (Winter, 1963), 2, pp. 159–171.

The ordering of human values upon which such choices must ultimately be based is a philosophical question into which I will not enter here. I shall assume that we have decided on social goals and shall then ask how we can translate these into practical recipes for deciding how much science we can afford.

The Budgetary Separation of Pure and Applied Science

I shall dispose of the question of what fraction of society's overall effort should go into "science as a whole" by arguing, along with many others, that "science as a whole" is a misleading idea. The basis for the claim which applied science makes on society is so different from that of pure science that lumping them together clouds the issue. Pure and applied science ought not to be viewed as competing for money.

Applied science is done to achieve certain ends which usually lie outside of science. When we decide how much we should allocate to a project in applied science, we at least implicitly assess whether we can achieve the particular end better by scientific research than by some other means. For example, suppose we wish to control the growth of population in India and suppose we have at our disposal $\$200 \times 10^6$ per year for this purpose. We could devote most of this sum to investigating fertility, to developing better contraceptive techniques, or to studying relevant social structures in some Indian village. Or, alternatively, we could use the money to buy and distribute existing contraceptive equipment, such as Gräffenberg rings, perhaps using some of the money as incentive payment to induce women to accept the technique. Which way we spend our money is a matter of tactics; evidently no general proposition can tell us how much of our effort ought to be spent on research rather than on practice in trying to achieve effective birth control in India. The scientific work that goes toward solving this problem ought to compete for money with alternative, non-scientific means of controlling the growth of population in India rather than with the study of, say, the genetic code. More generally, where a piece of research is done to further an end which society has identified as desirable, support for this type of scientific work should be considered as part of the bill for achieving the end, not as part of the "science budget". Only that scientific research which is pursued to further an end arising or lying within science itself should be included in our " science budget ".

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[2] "The Complexity of Scientific Choice: A Stocktaking", *Minerva*, II (Spring, 1964), 3, pp. 343–359.

This view has become quite popular in many recent discussions of the subject.[3] It is appealing to the scientist because setting support for applied science outside the science budget reduces the latter enormously—from $\$16 \times 10^9$ to perhaps $\$1 \times 10^9$. At this level the whole question of choice between scientific and non-scientific activities becomes much less significant.

But this stratagem is not as clearly justified as it appears at first sight. Ruling applied science to be part of the budget of non-scientific activities, not of the scientific budget, does not eliminate competition between applied science and basic science. Applied science requires at a secondary level, by and large, the same kind of people as does basic science. Building a large accelerator engages electrical engineers who would otherwise be available to help design control systems for rockets. In allocating support for a given applied science, one must keep in mind the effect of such allocation on basic science, and in supporting basic science, one must keep in mind the effect on applied science. Edward Teller has argued that because of the great emphasis on basic sciences in our universities, we have created an atmosphere that is uncongenial to applied science. He insists that our important applied scientific undertakings suffer because we tend to direct our best talents to basic science, our not-quite best to applied science. Though Teller's contention is difficult to prove, my own experience supports his view.

A second difficulty is that the aim of any given branch of applied science tends to become diffuse as time goes on. The scientific work of any of the large " mission-oriented " government agencies started out specifically to further the mission of the agency. But as time has passed, these clearly defined, " mission-oriented " goals of applied scientific work have become fuzzy. Byways that originally were germane to the mission flourish—an investigation that began as a promising approach to solve an applied problem, 10 years later becomes an interesting study pursued for its own sake, yet it continues to be described as " applied science ". Thus to leave applied science out of the science budget would leave out a large amount of research which was at one time motivated by an extra-scientific or applied end, but which is now pursued primarily because it is scientifically interesting to those carrying on the research.

Finally, the motivation for basic science is itself often less than pure. Is nuclear structure physics done to further science or to help build reactors? Is the structure of natural products pursued as a challenge to scientific virtuosity in organic chemistry or because out of such studies will come the knowledge of enzyme action which ultimately will lead to control of metabolic disorders? Thus consideration of support of basic research completely apart from applied research is not as clearly defined a proposition as many proponents of this position hold.

Nevertheless, I believe the *general* principle of not considering the budget for applied research as part of our " national science budget " and including only basic research in it has one overriding advantage. By allocating funds to any applied research as a certain fraction of the budget of the (usually non-scientific) activity to which the research is intended to contribute, we keep straight our reasons for supporting the applied research. What this fraction should be must depend on internal criteria—such as, do we see ways of making progress, or are good research workers available? It probably should also depend on the impact that support of that field will have on neighbouring basic fields.

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[3] A particularly cogent presentation of this position is made by Stephen Toulmin, *op. cit.*

The fraction of effort that goes into achievement of a broad end—like aid to underdeveloped countries or national defence—by scientific research instead of by non-scientific action can hardly be decided entirely by the scientists. The scientific approach to solutions of difficult social problems is becoming increasingly popular. Yet in at least some proposals for action of which I am aware—notably in foreign aid and control of world population—it seems to me that excessive claims were made for science. Scientists alone, when asked to judge how to solve a complex social problem, more often than not recommend more science—just as high-energy physicists, when asked to recommend a programme in basic science, will ask for more high-energy physicists or oceanographers for more oceanography. To overstate the capacities of scientific research as a technique for settling difficult social questions is no more sensible than it is to understate them. Thus, just as I have argued that scientific panels, judging how much money should be allocated to one branch of science rather than to another, should include representatives of neighbouring branches of science, so a panel determining how much scientific research rather than "engineering" or "production" will best achieve a certain non-scientific end should include non-scientists as well as scientists.

Support for Basic Science as a Branch of High Culture

I have argued in the foregoing that applied and basic science should have separate budgets and that the budget for applied science should be set as a certain fraction of the effort allocated to the end (usually non-scientific) which applied science furthers. To this extent I have avoided the problem of choice between "science as a whole" and other human activities by denying the usefulness of the concept "science as a whole". This still leaves the question of basic science—the science which cannot be justified by any reason except that it satisfies human curiosity. Are there some broad social ends, outside of basic science, which basic science serves, and to which its budget can be tied?

Obviously, some parts of basic science are important to applied science: in my view a much larger fraction of basic science is germane to applied science than many of my basic scientific colleagues are willing to concede. The bulk of the biological sciences is, in a sense, applied. For example, the most recondite and ingenious elucidation of the genetic map of *E. coli* is germane to the whole question of genetic abnormalities. (I often find it amusing to argue with my biologist friends that most of what they do is applied research—that the important distinction in a field of science intrinsically so close to human affairs as is biology is not between "applied" and "basic" but between "intelligent" and "unintelligent".) Or again, plasma physics, a purely basic science, is central to thermonuclear research, an applied science which is pursued because we wish to enlarge mankind's energy resources.

It is natural to propose that such basic research receive a certain fraction of the resources going into the applied research which it underlies. Every good applied research laboratory allocates to basic research a certain fraction of the resources allocated to it for its related applied research. The ratio of basic to applied research often is very high and is usually highest in the applied laboratories which have had the most success in accomplishing their technological mission. What I suggest is that on the national scale, also, basic research be considered as a fixed charge on the applied research effort, wherever the basic research is intended to contribute to a field of applied science. In making an assessment of relevance, I would incline toward a broad interpretation: for example, I would consider the case of most research in biology as a proper overhead charge to be assessed against the resources allocated to agricultural and medical research.

But what about those fields of basic research, a few of them very expensive, which are really very remote from any applied scientific problems, which are pursued primarily because the researchers find the science intensely interesting, often because the findings in this field are likely to illuminate neighbouring branches of basic science? To what can we tie the allocation of effort for such activities?

This is the most puzzling of all the questions concerning public support of science and any proposed solution must be put forward most tentatively. For basic science of this kind is primarily a somewhat disinterested intellectual activity, in the same sense as are music, literature and art. Indeed, the analogy between the creative arts and this purest kind of basic science is sufficiently great to suggest that, insofar as it must make the choice, society might choose between the pure basic sciences on the one hand and the creative arts on the other. In allocating support for the purest basic research, our allocations for the other creative activities of man might be taken as our guide.

There are many analogies between the purest basic research activity and artistic activity. Each is an intensely individual experience the effect of which transcends itself. The product of each is immortal—the theory of relativity, just as surely as *Hamlet* or the Mona Lisa. Each is concerned with truth—the highest of human manifestations—the one with scientific truth (which deals with the regularities in human experience), the other with artistic truth (which deals with the individuality of human experience).[4] Each enriches our life in unmeasurable though highly significant ways. Each belongs not only to its creator or discoverer, but to all mankind.

In a competition for support between pure science and the arts, I see two major arguments—one that supports the claim of science and the other, the claim of art. The argument that favours science (aside from the obvious one, to which I shall return, that even the remotest pure science may eventually have practical application) is that scientific truth, being based on what we observe in nature, is publicly verifiable, whereas artistic truth, not subject to the same kind of control, is not publicly verifiable. Artistic critics disagree just as often as they agree. They have no objective and impartial arbiter, nature, to say what is true and what is not true. The truth of science, on the other hand, is rigorously and publicly tested by experiment or by observation or, in the case of mathematics, by logic. Scientific criticism weeds out scientific nonsense more efficiently than artistic criticism weeds out artistic nonsense because, ultimately, science is monitored by a universal and approachable critic, nature, whereas art has no comparable critic. Scientific research and thought, in their mutual and ruthless criticism which reach ever more strongly towards a whole consistent structure, are embedded in what Michael Polanyi has called the " Republic of Science "[5]—the entire scientific community whose mutual interaction is governed by rules of scientific conduct that are themselves laid down by nature, the great scientific lawgiver. The republic of science forces science to be a responsible undertaking, at least in the sense that what science does is true and, in some approximation, true forever. The corresponding republic of the arts has no such final arbiter that can force art to be as responsible as science. In so far as public support ought to go for the more responsible undertaking, the purest science in this regard merits more support than do the arts.

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[4] This point was illuminated for me in Barzun, Jacques, *Science: The Glorious Entertainment* (New York: Harper & Row, 1964), p. 227 *et seq.*

But there is another argument which at present favours the arts. Pure science—that is, science which does not have foreseeable practical applications, such as elementary particle physics or cosmology—is by and large an arcane enterprise which is appreciated mainly by its practitioners. The arts, on the other hand, are generally less restricted in their audience: many more people in the world today can gain enjoyment from listening to Beethoven's *Ninth Symphony* than they can from reading Schroedinger's paper on quantisation as an *eigen*-value problem. Granted that the intellectual delight experienced by the creator in pure science matches that of the creator in art, the direct products of the latter's efforts at present probably give more enjoyment to more people than do the products of the former. Of course, in so far as even the purest science may eventually result in practical applications, it too affects the public at large; but we are speaking here of the science whose practical application is minimal.

The well-paid pure scientists among my friends will undoubtedly object to being converted into scientific bohemians shivering in poorly heated garrets. But I don't think pure science is doomed to that poor an existence if our society decides, even now, to support it on about the same scale as it supports the arts. It is true that the arts are supported poorly by government, but the total paid by the society, *i.e.*, private individuals and associations, governments, local and federal, for the arts is not negligible and the support is growing. In estimating the total support we give to the arts, we must include the value of theatre admissions, the value of books, better magazines and good records, the total that goes to our performing arts, as well as the direct subsidies in the form of grants to creative artists. The total spent by the United States on all activities that one way or another are concerned with the arts amounted in 1960 to around $2,500 million.[6] Only a fraction of this amount is spent directly by the federal government but this is not relevant. Pure science, unlike music or literature, produces no directly saleable commodity and so if it is to be supported at all by the public it must be supported by the public through its government.

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[5] Polanyi, M., " The Republic of Science: Its Political and Economic Theory ", *Minerva*, I (Autumn, 1962), 1, pp. 54–73.

Moreover, it seems likely, with the increase in leisure, and the decrease in the amount we spend on armaments, that a larger and larger fraction of our national income will go into the arts. Voices have been raised favouring a National Arts Foundation, paralleling the National Science Foundation. To make of pure science an avenue for expression of our creative intellectual energy, quite parallel to August Heckscher's[7] proposal to make of the arts such an instrument, strikes me as highly appealing. This latter viewpoint was stated eloquently by N. N. Semenov,[8] the Soviet chemist; he visualises science in the world of the future being appreciated and practised as widely as are the arts in the world today—every man a scientist, to the extent of his intellectual capacity.

I put forward the idea that the purest science be supported in the same spirit and at roughly the same level as the arts as only one among several possibilities. The arts, after all, are not the only non-scientific activity which gives deep intellectual or spiritual satisfaction. For example, religion even today gives great spiritual satisfaction to many people—in our country to many more than do the arts or sciences. And indeed, a case can be made for using the level of support of religion instead of art as a yardstick for how much pure science our society ought to support.

And yet, despite the analogies between science and art, or between science and religion, the idea of relating the degree of support of one to the degree of support of the other is somehow forced and artificial and not really satisfactory. In the long run how much our society is going to spend on basic science depends upon the extent to which non-scientists develop the intellectual power and taste to appreciate, if not to discover, science. The question then is: is it really likely that society will develop so congenial an attitude towards science—say as congenial an attitude as it now displays towards the arts or religion—that it will support the basic scientist at the level he thinks he needs?

Most scientists believe that society will be missing something very important should it *not* develop such an attitude towards pure science. Every scientist knows that much of the satisfaction he derives from his scientific career comes not only from his own original discoveries, but also from the thrill he experiences when he understands, for the first time, someone else's great discovery. My own experience during the past half dozen years illustrates the point. During these years, at least five major discoveries have been made in physics: the Mössbauer effect, the overthrow of parity, the laser, the superconducting magnet, and the SU_3 symmetry in strong interactions. After each of these discoveries I blessed my decision to study physics, since only because I knew some physics could I experience the unique intellectual satisfaction that appreciation of a discovery, almost as much as the discovery itself, affords.

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[6] Estimate by A. Mitchell of Stanford Research Institute, as reported by *Business Week*, 19 January, 1963, p. 68.

[7] Heckscher, August, "The Arts in the 1980s", a lecture at Oswego State University, Oswego, New York (1964).

[8] Semenov, N. N., "The World of the Future", *The Bulletin of the Atomic Scientists*, XX (February, 1964), pp. 10–15; the same idea was also expressed by George Bernard Shaw in *Back to Methuselah*.

One need not be a great intellect to appreciate a scientific discovery, at least enough to give one real satisfaction. I would guess that all those intelligent enough to take a university degree could learn enough to appreciate some branch of science: if not the most sophisticated parts, then at least the simpler parts. Nor is it necessary for all the public to understand all of basic science. Just as science itself has fragmented under the pressure of the information explosion, so I visualise that " lay-scientists " would also form somewhat separate communities: perhaps there might develop the equivalent of " molecular biology fan clubs ", " high-energy fan clubs ", " oceanography fan clubs ", even as we now have amateur astronomers, radio " hams " and hi-fi enthusiasts.

To educate so many people to a point where they can achieve a sense of participation in the march of science poses a major problem. The scientists themselves will have to spend much effort conveying their message, in intelligible terms, to the rest of society. They will have to deal sympathetically (much more so than I think they do now) with the scientific popularisers and with the scientific educators. If the scientists and their para-scientific associates are unable to convey this sense of scientific adventure to the community that supports them, I cannot see how the purest basic research can, in the long run, expect to receive the support it will demand in the future.

The problem faced by the future scientists has been stated by Professor Eugene Wigner as follows:

> . . . we all hope that the present competitions for the most powerful military posture will become unnecessary soon—perhaps in 10 years, perhaps in 20 years. Quite likely, not only will the present unquestioning support of science cease then; it will be replaced by distrust and even unpopularity. Nobody likes his companions and helpers of a past life from which he has turned to a better one. What will be the role of science then, where the scientist will be no longer a source of power of the government, after having been pampered so long, is not entirely pleasant to contemplate. However, it may be useful. Science that is useless in the sense that it does not help to satisfy other cravings, is still one of the noblest endeavours of man; it would be most pitiful if mankind turned away from science just when it will have the leisure to pursue science in its more noble form.[9]

Support for Basic Science as an Overhead Charge on Applied Science and Technology

I confess to a residual scepticism about our society acquiring this sophistication in the short run, which means, for the working scientist, the years until his retirement. It is probably utopian—as much as Shaw's *Back to Methuselah*—to expect every man in the street to become an amateur scientist or even a science fan.

Thus, much as I hope that our society will acquire this scientific sophistication, it seems clear that in the near, as opposed to the distant, future we shall have to present a more realistic claim on society's support of basic science done for the sheer intellectual pleasure it affords its practitioners. I therefore return to my earlier suggestion that basic science in fields clearly relevant to applied science be viewed as an overhead charge on that particular applied science—that is, against the political mission the applied science is intended to accomplish. I would extend the idea and urge that the purest basic science be viewed as an overhead charge on the society's entire technical enterprise—a burden that is assessed on the whole activity because, in a general and indirect sort of way, such basic science is expected eventually to contribute to the technological system as a whole. In some cases, the help will turn out to be direct, as when a discovery in cosmology illuminates a point in nuclear structure physics; in more cases it will be indirect as when a professor, whose research is in an abstruse field of mathematics, inspires a young engineering student with the beauties of the classical calculus of variations.

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[9] " Prospects in Nuclear Science ", an address delivered at the Twentieth Anniversary Celebration, Oak Ridge National Laboratory, 4 November. 1963.

Some such view of the relation of the purest basic science to the entire technical enterprise was implicit in Executive Order 10521 issued in 1954 by President Eisenhower concerning the terms of reference of the newly founded National Science Foundation:

> As now or hereafter authorised or permitted by law, the foundation shall be increasingly responsible for providing support by the federal government for general-purpose basic research through contracts and grants. The conduct and support by other federal agencies of basic research in areas which are closely related to their missions is recognised as important and desirable, especially in response to current national needs, and shall continue.

From this point of view one has further reduced the dimension of the problem of how much " science " shall we support. Applied science and engineering have already been ruled to be outside the " science as a whole " budget, inasmuch as they are a means of achieving a politically defined mission. Basic science, which is closely related to an applied science (such as biology, *vis-à-vis* medicine), is an overhead assessed against the related applied science, and therefore its level of support is again tied closely to a politically defined end. And finally, the purest basic science, viewed as an overhead against the entire enterprise, would, in analogous fashion, receive support at a level determined as a fraction of the entire remaining technical enterprise. What this fraction should be would itself be a political decision—but if all such research is supported by a National Science Foundation, as suggested by the Executive Order of President Eisenhower, this political decision would amount each year to setting the budget of the National Science Foundation. Of course this political decision would be influenced in part by the public's attitude towards science; but it would also be influenced by the attitude of legislators who are probably more inclined towards science than is the general public, since so much of the business of national legislative bodies now involves science and engineering in one way or another.

Where do the criteria of choice I proposed in my previous paper fit into such a scheme? As I see the matter now, they would be used both by mission-oriented agencies in making administrative decisions with respect to different kinds of basic science and by a body with very broad terms of reference, independently of any technological task such as those given to the National Science Foundation in the United States, in choosing between different basic fields. Within each allocation of funds made for a politically defined task there will always be more claimants than there are funds and choices will still have to be made. The beauty of the idea of basic research as a " scientific overhead " is that it reduces the size of each allocation of funds for scientific research to a more manageable proportion.

Thus I have turned a full circle: I began by asking how much " science as a whole " our society could afford. In developing my views, I have successively reduced the magnitude of science which competes with society's other activities, first by ruling the costs of applied science to be overhead charges on the tasks it sought to further; secondly by ruling the costs of mission-related basic science to be an overhead charge on mission-related applied science; and now by suggesting that the purest science be an overhead on the entire technological system. This is not to say that I object to the view of " science as culture ", a view which places science *per se* directly in competition with other activities of the society. It is merely that, in the short term, basic science viewed as an overhead charge on technology is a more practical way of justifying basic science than is basic science viewed as an analogue of art. Until and unless our society acquires the sophistication needed to appreciate basic science adequately, we can hardly expect to find in the admittedly lofty view of " science as culture " a basis for support at the level which we scientists believe to be proper and in the best interests both of society and of the scientists.

SCIENTIFIC CHOICE AND BIOMEDICAL SCIENCE

ALVIN M. WEINBERG

I CONTEND in this paper that of all the sciences now supported by our society, biomedical science ought to stand first. We are, or ought to be, entering an age of biomedical science and biomedical technology that could rival in magnitude and richness the present age of physical science and physical technology. Whether we shall indeed enter this age will depend upon the attitude toward Big Biology adopted by biomedical scientists and governmental agencies that support biology. Whether the age of Big Biology will be truly rewarding will depend on the common sense and integrity of all who participate in this adventure.

The Ongoing Debate

The scientific-political world has been debating scientific priorities with growing zeal during the past three or four years; yet in this debate the voice of the biologist has been rather mute. The public debate began, informally, with a number of essays on scientific choice by American and English authors.[1] Since then the debate has become more formal and quite widespread. For example, in the United States, the Committee on Science and Public Policy (C.O.S.P.U.P.) of the National Academy of Sciences has sponsored reports by groups representing different branches of science; these reports summarise the achievements, promise and needs of particular branches of science. Such " planning reports " on ground-based astronomy [2] and chemistry [3] have already appeared. Similar reports on physics, computers, mathematics and botany are being prepared. In the biomedical sciences a comparable effort under the leadership of Professor Philip Handler is just getting under way. Other reports such as those on earth sciences [4] and high energy physics [5] have also been published.

Despite the value of these formal reports, I think it is important that the informal debate on scientific priorities continue. Formal reports delineating the achievement and promise of various fields all tend to be isomorphic. It makes little difference whether the field is astronomy, physics, or computers: its achievements have been outstanding, its promise superb and its needs and tastes very expensive. Nor is this surprising. Each report is prepared by dedicated members of a particular scientific community whose passions and aspirations, as well as knowledge, centre on a single field. The very reasonable theory underlying the preparation of these reports is that each field should put its very best foot forward. Judgements among the fields would then be made by a higher body, like the President's Science Advisory Committee, that represents many different scientific fields.

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[1] Carter, C. F., " The Distribution of Scientific Effort ", *Minerva*, I, 2 (Winter, 1963), pp. 172–181; *idem*, Letter to the Editor, *Minerva*, II, 3 (Spring, 1964), pp. 382–383; Dedijer, Stevan, Letter to the Editor, *Minerva*, III, 1 (Autumn, 1964), pp. 126–129; Maddox, John, " Choice and the Scientific Community ", *Minerva*, II, 2 (Winter, 1964), pp. 141–159; Toulmin, Stephen, " The Complexity of Scientific Choice: A Stocktaking ", *Minerva*, II, 3 (Spring, 1964), pp. 343–359; Weinberg, Alvin M., " Criteria for Scientific Choice ", *Minerva*, I, 2 (Winter, 1963), pp. 159–171; *idem*, Letter to the Editor, *Minerva*, II, 3 (Spring, 1964), pp. 383–385; *idem*, " Criteria for Scientific Choice II: The Two Cultures ", *Minerva*, III, 1 (Autumn, 1964), pp. 3–14.

[2] *Ground-based Astronomy: A Ten Year Program*. A Report prepared by the Panel on Astronomical Facilities for the Committee on Science and Public Policy of the National Academy of Sciences (Washington: National Academy of Sciences—National Research Council, 1964).

[3] *Chemistry: Opportunities and Needs*. A Report on Basic Research in U.S. Chemistry by the Committee for the Survey of Chemistry, National Academy of Sciences—National Research Council (Washington: National Academy of Sciences—National Research Council, 1965).

[4] *Solid-Earth Geophysics: Survey and Outlook*. Panel on Solid-Earth Problems of the Geophysics Research Board and Division of Earth Sciences, National Academy of Sciences—National Research Council (Washington: National Academy of Sciences—National Research Council, 1964).

[5] *Report of the Panel on High Energy Accelerator Physics of the General Advisory Committee to the Atomic Energy Commission and the President's Science Advisory Committee*, TID-18636 (Washington: U.S. Atomic Energy Commission, Division of Technical Information, 1963).

Actually, the political process out of which flows our ordering of priorities does not work that neatly. Though the Science Adviser carries great weight, Congress and the separate government agencies must also be reckoned with and their views are harder to bring into focus. Interpretative and philosophic analyses of the problem of scientific choice, particularly judgements as to relative priority, will therefore remain important. Such judgements, by the nature of things, can hardly be other than individual opinions. Out of such individual views and opinions is fashioned a climate of thinking, an intellectual environment, which impinges in countless small ways on those in Congress and in the agencies who make scientific policy.

Some such view of the nature of the problem of choice, as viewed by Congress, was implicit in the response by the Committee on Science and Public Policy to two questions asked recently by the Subcommittee on Research and Development of the House Committee on Science and Astronautics. In effect, the Chairman of the Subcommittee, Congressman Daddario asked first, how much science our society should support; and second, how should the total science pie be cut? Rather than hammering out a weak consensus to such loaded questions, Professor George Kistiakowsky, former Chairman of C.O.S.P.U.P., asked each member of the committee to prepare an essay for which he alone was responsible, although each essay was criticised by other members of the group. This way of dealing with a question of public policy preserves the congressional tradition of eliciting many different opinions in arriving at a course of action. The collection of 15 essays on scientific choice is, I believe, a useful contribution to the debate on allocation of resources to science.[6]

The Argument for Biomedical Research

Any judgement as to the relative worth of any field of human activity involves an assessment of how that activity bears on human values. In particular, we support large-scale science because, in one way or another, we believe that out of large-scale science will come human benefits or values. Now the *value* of science cannot be determined from within science. It is a venerable philosophic principle that the value of any universe of discourse must be judged from outside that universe of discourse. It was for this reason that, in an earlier article on scientific choice,[7] I urged that large-scale public support be given a field of science *only if* it rated well with respect to what I called " external criteria ". These I identified as technological merit (meaning bearing on related technology), scientific merit (meaning bearing on related fields of science) and social merit (including national prestige, culture, etc.).

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[6] *Basic Research and National Goals. A Report to the Committee on Science and Astronautics, U.S. House of Representatives, by the National Academy of Sciences* (Washington: Government Printing Office, 1965), ix + 336 pp.

Two of the papers by members of the Committee on Science and Public Policy—" Federal Support of Basic Research: Some Economic Issues " by H. G. Johnson and " Scientific Choice, Basic Science and Applied Missions " by A. M. Weinberg—were reprinted in *Minerva*, III, 4 (Summer, 1965), pp. 500–523.

Of all the bases for claiming large-scale public support of a scientific activity, the possibility of alleviating human disease through such activity is one of the most compelling. Of all the sciences, the biomedical sciences are most directly aimed at and most relevant to alleviating man's most elementary sufferings—disease and premature death. There is urgency of the most excruciating kind in getting on with this job. The assault on human disease, insofar as it may result in alleviation of immediate everyday human suffering, has an urgency about it comparable to the urgency with which a nation prosecutes a war. Indeed, I would draw an analogy in this regard between war-time research in physics and present-day research in the biomedical sciences.

This claim to urgency can hardly be matched by any of the other great fields of natural science. Certainly those fields that base their claims to support primarily on the promise of enlarging the human spirit have, to my mind, a less valid case for *urgency* than do those fields that base their claim on the possibility of curing or preventing human disease. SU(n) symmetry is magnificent and soul-satisfying to those who understand it; a cure for leukaemia is more immediate in its benefit to mankind.

Are the biomedical sciences that relevant to the conquest of disease? To an applied scientist like me, this question seems absurd. What strikes an observer most about modern biology is how the new viewpoints have unified the subject. The genetic code appears to be universal. The dogma of protein synthesis—DNA, messenger RNA, transfer RNA, protein—seems to be valid in almost every life form. The same 20-odd amino acids build proteins in bacteria, in mice, and in men. This unity suggests that most of what we learn about biological mechanisms in almost any animal is likely to have ultimate medical applications, whereas the same degree of relevance to application cannot be claimed for large parts of modern physics, or astronomy, or mathematics. In the biomedical sciences the distinction between pure and applied is rather irrelevant. The distinction is better made between intelligent, imaginative research and unintelligent, plodding research. As a matter of tactics, I have therefore argued that all of the biomedical sciences be viewed as applied science, even though I know that calling some of my good friends who consider themselves to be basic biological scientists " applied " scientists hardly endears me to them. Yet from the point of view I am discussing here—the validity of the biomedical sciences' claim to urgency and therefore the validity of their claim to large-scale support from society—the position of biology is far stronger if it regards itself as fighting the war against disease instead of the war to enlarge the human spirit, worthy as the latter is.

If the biomedical sciences are viewed as applied sciences, aimed at alleviating disease, then in assessing their priority they should be judged not so much against other branches of science that are not aimed at the same goal but rather against alternative means of alleviating disease. The most obvious such alternative is medical practice, including treatment centres, hospitals, medical education, nursing care, etc. And indeed, I believe there is evidence of competition between the demands of medical practice and the demands of medical research. I refer to the frequently quoted statistics showing that the relative number of A students in first-year medical school in the United States fell from 40 per cent. to 13·4 per cent. during the period from 1950 to 1960.[8] Although it is hard to document, I have always believed that at least part of this loss in quality was a consequence of the favoured position of the graduate student in biomedical research as compared with his counterpart in medicine. The United States Government has made fellowships available for the research student but, with few exceptions, not for the medical student. I expect this situation to change in the United States as a result of such studies as that by the President's Commission on Heart Disease, Cancer and Stroke, which bring the country's attention to the need for more medical practice. My own view is that we need more biomedical science *and* more medical practice and that the two, taken together, deserve very high priority in the allocation of resources.

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[7] Weinberg, Alvin M., " Criteria for Scientific Choice " *Minerva*, I, 2 (Winter, 1963), pp. 159–171.

The Prospect of Returns from Biomedical Research

Relevant as is the aim of a science to achievement of a recognised human value—in the case of biology to the elimination of human disease—this can only be a partial justification for large-scale public support. Before any scientific field can expect support on a very large scale it must be at a stage where large-scale public support is likely to produce useful results. Anyone who claims that biomedical science should become our number one scientific priority must show that this field is likely to give fair return for support received.

In this respect the situation in the biomedical sciences at first sight seems to stand between certain of the physical sciences and the behavioural sciences. Judging by the criterion of direct relevance to human welfare, any ordering would almost surely place the behavioural sciences at least on a par with, if not above, the biomedical sciences; the more abstract physical sciences would almost surely rate below these. Judging by the criterion of intellectual readiness for exploitation—*i.e.*, whether it is a lack of large-scale support which is mainly responsible for restraints on progress—abstract physical science, like elementary particle physics or astronomy, is probably ahead of biomedical sciences and the behavioural sciences are much farther behind. This at least is the view one would gather from the strength of the plea for support made by the physical scientists, compared with the relative weakness of the plea we hear from the biomedical scientists. I think, however, that the biomedical scientists understate their case.

To begin with, the war on human disease is a tangible war—more tangible, say, than our efforts to enlarge the human spirit—and it should be fought with the same attitudes we adopt when fighting a real war. We expect smaller returns per dollar expended when fighting a war than when carrying on a less crucially important activity. So I would argue that, because of the importance of each victory in the battle against disease, we ought to be willing to get less per dollar spent on biomedical research than we are willing to get from expenditures on the more remote fields of science. We should stop putting more resources into the enterprise only when we have reached a stage of negative returns—when more resources *reduce* the total useful output—not merely raise the unit cost of an increased total output.

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[8] Wiggins, Walter S., *et al.*, " Medical Education in the United States ", Journal of the American Medical Association, CLXXVIII, 6, 11 November, 1961, p. 601.

I believe the biomedical sciences are not near the stage where additional large-scale support will *reduce* the total output of the entire enterprise. It is apparent even to the most casual observer that we are beginning to understand many of the life processes which have been mysteries for so long: the revolution in molecular biology, including the unravelling of genetic codes, determination of the structure of proteins and insights into enzyme action; or the beautiful elucidation of the mechanism of nerve action; or the new insights into the genetic control of immune mechanisms; or the extraordinary implication of viruses in some cancers, notably animal leukaemia, although their role had long been suspected. One can hardly believe that the many fruitful points of departure uncovered during the past decade are anywhere close to being exploited; or that, if more well-trained, well-supported, investigators were set to work, new and startling points of departure would not emerge.

Moreover, the biomedical sciences can be force-fed, even more than they are now being force-fed. More money for biology has raised the salaries of biologists, at least in the United States, so that now the biologists enjoy an unaccustomed affluence. Though this state of affairs annoys administrators, particularly of multi-disciplinary laboratories where disciplines use each other's salary schedules as ratchets, the overall effect as far as biomedical science is concerned is, on balance, good. More intelligent young men and women are attracted to well-paid careers than to poorly paid ones. In the United States such force-feeding of a discipline in the past has produced results. For example, the Atomic Energy Commission, by pouring money into nuclear research, caused nuclear research to flourish and encouraged many young science students to go into nuclear research. Or again, the Atomic Energy Commission and the U.S. Department of Defense deliberately established about a dozen interdisciplinary materials research laboratories; though it is too early to say positively, my impression is that materials research in the United States has profited by this action.

The Absorption Capacity of Biomedical Research

There are other reasons, intrinsic to the changing style of research in biology, why more money will be needed. Most obvious is the growing cost of equipment. A modern electron microscope now costs $40,000 and more and more cellular biology seems to depend on the electron microscope.

Even now attempts are being made both at Oak Ridge National Laboratory and at Argonne National Laboratory to develop an electron microscope with a resolution of 1A°. Such a device, if successful, would enable one to identify individual atoms in biological molecules. It could cost several million dollars.

But there are other, possibly subtler, reasons why biological research is becoming more expensive and is requiring more people. In earlier times, when biology was Little Science *par excellence*, biologists were content to look only at those problems that could be handled by the style of Little Science. Genetics was done with fruit flies, with their large chromosomes, because fruit flies are inexpensive, not because fruit flies are as much like man as are mammals. Those questions that required large protocols of expensive animals were answered poorly or not at all—not because the questions were unimportant but because to answer them was expensive and required the style of Big Science which was so foreign to the biologists' tradition.

But this is changing, in part at least, because the Big Scientists from neighbouring fields have taught the sin of Big Science to the biologists. Perhaps the best known example of the drastically changed style of some biological research is the large-scale mouse genetics experiment of Dr. W. L. Russell at Oak Ridge. For the past 16 years Russell has been studying the genetic effects of ionising radiation in a mammal, the mouse. Since mutations even at high dose rates are so rare, Russell uses colonies containing 100,000 mice. To perform such experiments takes much money and many people; and yet it seems impossible to visualise any other way of obtaining the data.

The problem of large protocols which Russell faced and the Atomic Energy Commission solved (at a cost of $\$10^6$/year for this single experiment) is one which arises in many other situations. The increasingly important matter of low-level physical and chemical insults to the biosphere will require many large experiments if we are to assess accurately the various hazards that now bombard us. Or take old age, the commonest " disease " of all: merely because the effects are subtle and often appear haphazardly, the study of aging requires large and expensive protocols. The tradition of the biologists, and it is a very honourable and desirable tradition, has been a niggardly one; biomedical research avoided expensive experiments even if expensive experiments were required to obtain reliable statistics. I believe that biology, while continuing its tradition of Little Science, shall have to accept also the style of Big Science and that, even though this is expensive, the biologists will find the public willing to support them.

There is another trend in the style of biology which will add to its expense. This is the increasingly interdisciplinary character of modern biology and, particularly, its increasing dependence on the techniques and methods of the physical sciences and even of the engineering sciences. A few examples, taken from our own experience at Oak Ridge, will illustrate these points. For example, in attacking the problem of radiation insult, we have mobilised biochemists, cytologists, geneticists, pathologists and biophysicists. Our dependence on disciplines even farther removed from biology is growing. Thus, our biochemists, notably Dr. G. D. Novelli and Dr. M. P. Stulberg, need large quantities of t-RNA, preferably separated into unique fractions, to study how amino acids are assembled into proteins. The problem in many ways is one in chemical engineering and some of the chemical engineers, particularly Mr. A. D. Kelmers, at Oak Ridge National Laboratory have pitched in to help. What the chemical engineers have done already strikes me as being rather impressive. They have been able to extract as much as 600 grams of pure t-RNA from 300 kilograms of *E. coli* by fractionating crude nucleic acids in a sodium acetate-isopropanol mixture followed by selective elution from a DEAE-cellulose column. They have then fractionated the specific t-RNAs by using a liquid ion exchange system based on quarternary ammonium compounds of the general sort developed at Oak Ridge National Laboratory in refining uranium ores. The resulting separations are superior to any that have been achieved by older methods.

Second, I mention, again from Oak Ridge experience, the exciting developments in zonal centrifugation applied to biology. For many years very high speed, very large, continuously fed centrifuges have been developed for separating the isotopes of uranium. Much of this work has been carried out at the K-25 Gaseous Diffusion Plant. Some four years ago, Dr. N. G. Anderson of the Oak Ridge National Laboratory Biology Division realised that such centrifuges, suitably modified, might separate cellular moieties on a larger scale than could be done with any other technique. And indeed, with the generous support of the National Cancer Institute and the Atomic Energy Commission, this is exactly what has happened. With these centrifuges Anderson has been able to detect virus-like particles in leukaemic blood more consistently than have most other investigators who do not have this tool available. I would expect Anderson's centrifuges to become widely used in biomedical research, even though some of his centrifuges cost as much as $45,000.

I could list many other instances of the growing interaction between the biological sciences and the physical and engineering sciences—for example, the technique of medical scintillation spectrometry which has become a medical specialty in its own right; or the wide use of computers in biomedical science; or, for that matter, the application of the methods of quantum chemistry to the attempts to understand the carcinogenic action of aromatic hydrocarbons. But I have given enough examples to bring out the main points: that biomedical science is becoming even more interdisciplinary; that the disciplines and techniques it draws upon are expensive; and that this will add to the expense of biomedical science.

The Division of Labour between Universities and Research Institutes

The changing style of biomedical research and its great and urgent expansion will affect the future organisation of such research. At present, a very large part of biomedical research is carried out at universities—institutions that are, or should be, committed to education at least as strongly as they are committed to research. University biomedical research must flourish and, to do this, it must grow. We shall have to maintain Little Biology as well as Big Biology and we shall have to produce many more trained biomedical scientists if we are to attack, with either style, the problem of human disease with sufficient urgency.

But much of the great expansion in biomedical research should take place in biomedical research institutes, many of which will be directly affiliated with universities, but many of which will not. For, as Professor Rossi put it so well in a recent issue of *Daedalus*,[9] the social ecology of the university is not as well suited to a massive attack aimed at a single goal as is the ecology of the research institute. In the first place, the traditional departmental structure of the university is poorly suited to interdisciplinary approaches. In the second place, in the university individuality and academic freedom are preciously guarded prerogatives and these are often incompatible with achieving success in tasks that require cooperation.

The ecology of the research institute has a different tone: it is more hierarchical, its members interact with one another more strongly, and it is interdisciplinary. In the individualistic, competitive university environment, genius flourishes but things go slowly because each genius works by himself with his own small group of students and assistants. In the less individualistic, cooperative institute environment, genius probably does not flourish as well but things go very fast because so many different talents can be brought to bear on a given problem. It is a place in which, however, a single, very able man can exert much more power and influence than he can in the university environment; it is a place where the whole is often much more than the sum of its parts.

If one accepts the proposition that biomedical science ought to be pursued with the same urgency with which we pursue military research, then the institute provides a better setting for such activity than does the university. In speaking this way I admit to being very much influenced by our own experience at Oak Ridge. There we have a prototype of a large biomedical institute: its central theme is the radiation insult to the biosphere. In pursuing this major theme, many disciplines are brought to bear. The enterprise is benevolently hierarchical; it is large; it is interdisciplinary; and I think it is effective.

I would therefore suggest that much of the big expansion in biomedical research ought to go toward establishing additional interdisciplinary institutes, like the Sloan-Kettering Institute, or the contemplated environmental health institute of the World Health Organisation. Certainly close connections with the universities are desirable; but I do not regard these as primary. The main job is to learn as much as possible in as short a time as possible to alleviate human suffering. In some cases this aim is furthered by close association with a university. I suspect that there are many cases where only a loose university affiliation is desirable.

Collaboration with the Physical Sciences: Financial Aspects

The coming age of biomedical science will impose on administrators of biomedical research a new and unaccustomed responsibility toward the physical sciences. I have already alluded to the increasing relevance of the physical sciences to the biomedical sciences. It is time for the community of biomedical science to recognise its dependence upon certain of the physical sciences and to assume a proper share of their support.

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[9] Rossi, Peter H., " Researchers, Scholars and Policy Makers: The Politics of Large Scale Research ", *Daedalus*, LXLII, 4 (Fall, 1964), pp. 1142-1161.

Support of certain parts of physical science has already been taken up by the biomedical sciences. For example, in the United States, the National Institutes of Health are now the largest single supporters of basic chemical research in the universities. But my impression is that such support tends to be somewhat constrained by narrow interpretations of relevance.

Research in many of the physical sciences—like structural organic chemistry, or X-ray and neutron diffraction, or even certain parts of solid state physics—is the proper concern of the biological sciences. The whole Watson-Crick development would have been impossible had it not been for major developments in the techniques of X-ray diffraction. Moreover, more and more of the world's leading biologists seem to be coming from the physical sciences: I mention, for example, Dr. Francis Crick, or Professor Seymour Benzer, or Professor Paul Doty, or Dr. Kenneth Cole. The debt owed to the physical sciences by the biomedical sciences is one of long standing and it is growing. It is now time for the biomedical sciences to begin repaying this debt.

The basic physical sciences in the United States are facing a major financial crisis. In the past they have been supported largely by three agencies: the Department of Defense, Atomic Energy Commission, and National Aeronautics and Space Administration. But the missions of these three agencies—defense, atomic energy and exploration of space—are not likely to receive increasing support; on the contrary, the United States in the past year has made the political decision to keep these agencies at about their present level, or even to reduce them somewhat. Thus the physical sciences, insofar as they are supported because they are relevant to the achievement of the missions of these agencies, are probably destined to receive relatively less support in the future than they have in the past.

But this predicament comes at the time when support for the biosciences should greatly increase and when the connections between the physical and the biomedical sciences become ever stronger. What is more natural than to ask the biomedical sciences to carry a fair share of the burden for supporting the many branches of physical science that are broadly relevant to the biomedical sciences? Such a plea from the hard-pressed physical scientist has justice on its side. The biomedical administrators, in their newly found affluence, should heed these cries from their colleagues in the physical sciences who have helped them so much for so many years.

Big Science and Little Science in Biomedical Research

Traditional biologists must surely recoil in horror at the advice given here:—to expand even at the cost of individual effectiveness as long as their total output increases; to break down their traditional disciplinary barriers and to adopt more of the institute, as contrasted to the university, style of research; to overcome their suspicion of the physical scientists; in short, to accept the new style of Big Science, in addition to the old style of Little Science.

If this is their reaction, they should be reminded that insofar as what they do is part of the war against human suffering their desires and tastes are not all that matter. Biomedical science is not done or, more importantly, not supported by the public simply because it gives intense satisfaction to the dedicated and successful biomedical research worker. It is supported on a really large scale because out of it have come means of eliminating man's infirmities. If a style that complements the traditional style is needed in order to build a much larger biomedical research enterprise, then this style will have to be adopted much as it hurts the sensibilities of those attached to traditional patterns of scientific organisation.

I have myself inveighed against the dangers of Big Science:—its preoccupation with the grandiose announcement rather than the great discovery; its substitution of money for thought; its over-abundance of administrators; its incompatibility with the educational process; even its inefficiency. As Sir Winston Churchill once said, "I do not unsay one word of this". But nothing I have said implies that I consider the style of Little Science to be obsolete. In urging more biomedical science, I plead both for more Big Science and for more Little Science.

Big Science, with all its dangers, does have a real place in the scheme of things. When the end to be achieved is important enough, and when the state of the science suggests that more support will lead to more results (and both these circumstances apply to biomedical sciences), then we are justified in going all out in our plea for public support. More than that, we have a responsibility to apprise the political leadership of the country of this belief. The coming age of biomedical science will not be an unmixed blessing for the biologist: he surely will fret at being involved in something big and unwieldy and at times inefficient. Nevertheless, as a responsible member of the human race who is sensitive to the purpose of enlightened human activities such as biomedical research, he will have to submerge his instinctive distaste for bigness in the interest of the welfare of humanity.

Sociology of Science[1]

NORMAN KAPLAN[2]

A quarter of a century has passed since Merton (1938) first published his pioneering study of science and technology in seventeenth-century England. More than a decade has passed since the appearance of the first textbooks, by Bernard Barber (1952), wholly devoted to the sociology of science, and some years have passed since Barber's (1956b) discussion of the trends in the sociology of science. In his Foreword to Barber's text, Merton (1952) noted and analyzed the relative neglect by sociologists of the sociology of science. The present review shows that sociologists continue to neglect this field. The sociology of science itself, however, has been literally blossoming in this past decade.

The virtual neglect of the field by sociologists is amply evident throughout this chapter, primarily by the scarcity of studies. No further detailed documentation is necessary but a few items might be noted in passing. For example: (1) The number of American sociologists explicitly interested in the sociology of science is extremely small: The hard core consists of perhaps a dozen, with another score or so interested in a peripheral fashion. (2) The introductory sociology textbooks, often considered (rightly or wrongly) a reflection of developments in the field, have practically ignored the sociology of science. Chinoy (1961, Ch. 16) stands as the sole major exception. A review of the major texts which have appeared in recent years shows almost no awareness at all of the existence of science as a social institution. In many of the leading texts there is not a single reference to the existence of science. (3) A review of the Ph.D. thesis titles listed in the *American Journal of Sociology* during the past decade suggests that, even with broadest definition of what constitutes the sociology of science, no more than an average of one dissertation per year could reasonably be classified as devoted to the sociology of science. (4) A review of the articles published in the major journals of sociology during the past decade shows a very small number devoted primarily to the sociology of science. In short, a review confined primarily to the contributions of sociologists to the sociology of science would not only be a very brief one, but would also omit most of the contributions that have been made to the field during the past decade.

What is the sociology of science? In the broadest sense, the sociology of science is concerned with the interrelations of science and society. How has sc[illegible]ed values, education, ci[illegible]ture, ways of life, political dec[illegible]d ways of looking at the world? How has society, in turn, influenced the development of science itself? These questions loom so large on the horizon today that many scholars have been unwilling to wait until the sociologists themselves become interested in the sociology of science.

There is no readily available and acceptable conceptual scheme which defines the boundaries or lays out the major theoretical questions and hypotheses in the sociology of science. Barber (1959, p. 223) suggested that Talcott Parsons' (1951, Ch. 8) discussion is perhaps the best available conceptual scheme, but even as "a guide to theoretical fundamentals" this treatment is considered unsatisfactory as a general framework for the whole of the sociology of science, however adequate it may be for some portions of it. Despite the absence of a unifying conceptual scheme, a review chapter such as this must implicitly suggest one, for the categories used to organize the material reviewed reflect decisions to include as well as exclude materials. The scheme is certainly a rudimentary one. It divides the review that follows into four major categories: (1) the nature of science; (2) the nature of scientists; (3) the organization of science; and (4) the interrelationships of science and society.

In the first section, the emphasis is primarily on science as a social system or social institution with its own distinct values, roles, and intra- as well as interinstitutional relationships. In the second category, a review will be presented of what is known of the social background, personality, motivation, socialization, and other social psychological aspects of the scientist.

The organization of science is subdivided into two categories. The first deals with the organization of scientific research at the laboratory level. Here are included studies of the increasing complexity of research organization, the development of new organizational roles such as that of the research administrator, and the growing concern to develop organizational environments which will promote productivity and creativity. The second part treats some of the new problems encountered and studied with respect to national patterns of scientific organization in different societies. Especially important are the problems of the support, planning, and control of science at the national level.

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[1] The Handbook of Modern Sociology, 1964, Rand McNally & Company, Chicago, Ill., pp. 852-881, Robert E. L. Faris, Editor.

[2] Grateful acknowledgment is made to the U.S.P.H.S. (United States Public Health Service), National Institutes of Health for a series of research grants (the current one being GM 09225-03) which have made it possible to explore some of the facets of the sociology of science discussed here. I also wish to acknowledge the help and critical suggestions of Harold J. Bershady, Beverly F. Porter, and Brenda R. Silver.

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The final section reviews some of the more significant developments in the increasingly intimate relationship between science and society. Science is rapidly losing its former insulated status. It is now in the forefront of many of the major decisions made in the political, economic, military, and social spheres. Whether one is concerned with the newly developing nations, the cold war, or the population explosion, one is actively concerned with the role of science and scientists.

Before proceeding it should be noted that this review departs from traditional practices for it does not include an explicit section on the historical antecedents of the sociology of science. These are described in the various works of Barber (1952; 1956a; 1956b; 1959). To discuss these antecedents in a brief space would hardly do justice to the richness of the available material. One point should be mentioned, however: The continued neglect by sociologists of the sociology of science has inevitably resulted in the neglect of some of the more important contributions of its earlier sociologists. While the author would agree with Barber about the significance of the contributions of Weber (1946), Marx (1935), Mannheim (1936), Znaniecki (1940), and others, he can only add that their suggestions and fruitful hypotheses have yet to be fully exploited in much of the work currently in progress. Rather than pay the usual lip service to the founding fathers, it will be left to the reader to judge for himself whether the sociology of science has benefited, and would benefit, from a closer reading of some of their works.

THE NATURE OF SCIENCE

Scientists actively engaged in research are not concerned particularly with the nature of science as such. They have been largely content to leave such broad questions to the historians and philosophers of science. Many scientists have argued that too much self-awareness and self-consciousness in following the formal description of how science is conducted would inevitably impede the progress of the research. Whether this is so or not, anyone who would study science from a sociological perspective must have some working conception of the nature of science.

One must be able to distinguish between the ideal pattern and the actual patterns which exist. It should be superfluous to point out that scientific research does not progress as it is described in a scientific publication. The textbook description of scientific method which leads one to expect an orderly progression from the recognition and definition of the problem, to the framing of hypotheses, to the empirical testing of these hypotheses, to their subsequent verification (or not), followed by the reformulation of hypotheses, and their reintegration with existing theory, and so on does not occur so neatly in real life laboratory situations. We all know that it is not so and yet sometimes we behave as if it were—a situation which may be all right for the man conducting the research but is less desirable for one who would *study* the man who is conducting the research.

Science has often been viewed as a monolithic entity, especially the corpus of the physical and natural sciences. Social scientists are well aware of the usual distinction between the developed and less-developed sciences, but sometimes forget that there are vast differences between some of the highly developed natural sciences and some of the newer and considerably less well-developed ones. Moreover, they tend to forget about the significant differences within a single broad science, such as physics, in which some of the newer subfields are as much underdeveloped as some of the social sciences.

Another problem is the question of basic versus applied research. This has played a large role in the discussion of scientists (Wolfle, 1959b), in the development of adequate statistics on the scientific effort, and in the formulation of national policies (National Science Foundation, 1957; Naval Research Advisory Committee, 1959), but has been largely unexplored by social scientists. The distinction is most frequently made in terms of the motivations and attitudes of the scientist—whether he does research for its own sake or to attain some particular end considered useful; e.g., a cure for a disease or a new clean bomb (Kidd, 1959b). Despite this large social component, most social scientists have been content to accept this distinction without much question and certainly without the considerable analysis required.

Sociologists and others who would study science as a phenomenon in its own right need to have a far more accurate picture of the reality of scientific endeavor. They do not need to become physicists or chemists, nor even to acquire the entire corpus of knowledge of a contemporary physicist or chemist, any more than they would need to become juvenile delinquents or lawyers or unemployed housewives to study any of those populations.

Those who would study science or scientists from a social perspective must know something about the technical aspects of science. Whether they lean most heavily on the philosopher of science (Nagel, 1961), the historian of science (Kuhn, 1962b), the scientist himself, or some combination of these is immaterial. Kuhn's (1962b) work is an especially important contribution in this context since he raised many new questions concerning the traditional views of science and its development. He challenged the usual notions of the cumulative nature of science, and, even more important, his distinctions between "normal science" and the revolutions in science have enormous, but as yet barely explored, implications for the sociology of science. The important thing is not to neglect the technical aspects of science in order to be able to decide how these affect and are affected by social and other "external" conditions and factors.

Science as a Social System

Just as Durkheim (1933) did for the division of labor, and Weber (1930) for the rise of capitalism, so Merton (1938) attempted to explain the origins of modern science. The development of modern science was aided and abetted by changes in religious and other values of the society. Behind the rules that developed for the conduct of science stood a system of moral imperatives, sanctions, and interrelated roles which helped to support and maintain the purely technical aspects of science as an ongoing activity. While the technical aspects are exceedingly important, as was noted above, one must also be alert to the extremely and equally important nontechnical factors which are often sociological in nature.

The work of Merton (1938; 1957b) and Parsons (1951) in laying out some of the distinctive features of science as a social system has probably exerted the most influence on contemporary research in the field. Barber (1952) not only brought them together and added some of his own comments, but also was instrumental in bringing their work to the attention of a wider audience.

In his now classic paper on "Science and the Social Order," Merton (1957b) suggested that there were four basic institutional imperatives for science. These were: universalism, communism, disinterestedness, and organized skepticism. All of these, Merton argued, were derived from, or related to, the technical demands of science. But these were singled out because of their moral aspect which gave them the characteristic of being more than technical norms. Merton readily admitted that these imperatives were derived largely from the writings and documents of the seventeenth century. Implicit in Merton's formulation of his four institutional imperatives is the idea that these have remained relatively unchanged from the time of their early origins. But this hypothesis needs to be reexamined (Kaplan, 1963b). West (1960), for example, found substantial departures from the classical position on moral values governing scientific research among a small sample of academic scientists.

Parsons (1951) treated the normative system of science in three different categories, some of which overlap Merton's (1957b) institutional imperatives. Parsons posited four basic norms relevant to scientific knowledge: empirical validity, logical clarity, logical consistency, and generality of the principles involved (Parsons, 1951, p. 335). These are primarily "technical" norms. When Parsons discussed the scientist's occupational role he did so in terms of his pattern variable configuration: universalism, affective neutrality, specificity, achievement orientation, and collectivity orientation (Parsons, 1951, p. 343). Parsons also talked of two norms which bind the scientist as researcher, namely tentativeness; and the acceptance of the validity of scientific findings which have been adequately demonstrated (Parsons, 1951, p. 353). The latter are clearly equivalent to Merton's organized skepticism and to universalism. Merton's other two imperatives may be found amongst Parsons' pattern variables.

It is, of course, possible to separate theoretically the values or norms of an institution from those of the participants involved, at least at an abstract conceptual level. The four basic norms of scientific knowledge posited by Parsons (1951) are binding on the map of science. His work must be empirically valid, logically clear and consistent, and general in terms of the principles involved. These are more or less technical rules simultaneously constituting limiting conditions and goals for the activities called science. They are also the desirable and, indeed, required attitudes which a scientist should display toward his work. In addition, these basic norms provide the criteria by which one's work is judged and evaluated.

The pattern variables, on the other hand, define the expectations specific to the role. So long as the basic norms of producing scientific knowledge are accepted, the role configuration is in some senses superfluous. For example, one would judge a work according to its empirical validity, logical clarity, and so on, without reference to the man's color, class, or other social attributes. Perhaps, though, the specific role attributes are essential because any particular individual has to be "reminded" of the need to be universalistic, affectively neutral, and so on, in his role as scientist, since in other roles these same criteria might not apply with equal force. One might also question Parsons' (1951) distinction between the scientist's occupational role (to which he attached a particular pattern variable configuration) and his designation of the scientist as researcher, to which he adds two additional, norms discussed previously.

A general question which emerges is the extent to which the norms posited by Parsons (1951) or Merton (1957b) are explicitly tied to the occupational role, or to some facet of that role. Is it possible to view these norms as essentially "free floating"? To what extent is the researcher role different from the occupational role of scientists? Is Parsons referring particularly to the occupational role within an institutional context, such as the role of the university professor? Questions may be raised also about the interrelations of a set of values for the system and for the participants within that system. Is one to assume, for example, that these particular values are the more strongly held by a scientist the more strongly he is integrated into the scientific social system? Is the scientist more or less integrated into the system at different points in time? Is the scientist more or less integrated in the period immediately after initial socialization or at a much later stage in his career? Are there differences in the way in which these values are accepted by participants in relation to differences in "the stage of development" of a science itself—for example, are sociologists more likely to overconform (or underconform) to these values (say, compared with physicists) because sociologists are relative newcomers to the world of science? Finally, one must question the strong suggestion that all scientists are peers, with an almost total neglect of the internal hierarchial structuring and the resulting relationships which occur whether these be in a university, industrial, or government laboratory.

A further point concerning the value system posited by Merton (1957b), Parsons (1951), and others is the extent to which these values are presumed to have remained unchanged since the seventeenth century. Merton in particular argued that they have hardly changed and pointed to certain deviations, and the reactions to these, which occurred during World War I, for example, as evidence of the continued strength of these norms. There have been few attempts either to study these values and norms in some broad systematic way or to develop them further along theoretical lines. In general, there seems to be unqualified acceptance of them by scholars who study science and scientists. For example, two recent books, one by Marcson (1960b) and the other by Kornhauser (1962), are based largely on the hypothesis that these values are still in existence and strong. In both books the central problem is viewed as the conflict between the values of science and the values of the organization, more specifically those of industrial organizations. Though the terms employed are not identical, in general the conflict is seen to be between the requirement for autonomy of the social system of science versus the requirement of control of the industrial organization. Further, the specific institutional imperatives of science are viewed as being threatened by those of industry. This applies particularly to the norms of communality (as Barber [1952] rechristened Merton's imperative of communism) and disinterestedness. Whether their analysis is correct or not, the point to be stressed here is that the values posited by Merton and Barber and Parsons have been fully accepted as those which prevail today, without any additional empirical verification or theoretical analysis.

The Communications System in Science

The institutional imperative of communism obliges the scientist to communicate his results freely and to abhor secrecy. Aside from the problems of restricted communication in matters of military security and in industry, where possibly a competitive advantage may be gained by withholding technical data, the imperative against secrecy appears to be superfluous. Scientists, especially those in the universities, seem well aware of the increasing significance of the "publish or perish" theme. The current "publications explosion" is viewed by many observers as an indication that too many scientists are rushing into print much too often (cf. Calder, 1961).

Surprisingly, there has been little discussion about the norms surrounding publication. A major exception has been the series of papers by Merton (1957a; 1961; 1963) on the conflicts over priority rights among scientists. But there has been no known systematic study of the norms pertaining to the precise timing of a scientific communication in relation to the stage of the research project; to the arrangement of names in multiple-authored papers; or to the assignment of publication credit where the original idea may have come from one man, but where the actual research experiment has come from several others, the analysis has been done by another, and the major writing job has been done by yet another man. A physicist (Reif, 1961) recently suggested that the competition is becoming much more intense in this arena, even for the "pure" scientist.

Clearly, the whole area of communications is a vital part of the social system of science. It is essential to remind oneself of this fact in the light of the tremendous upsurge of interest in the technical aspects of communication resulting from the continuing publications "explosion." For example, as a recent government report stated:

> *Chemical Abstracts* in 1930 contained 54,000 abstracts; a private subscription cost $7.50 per year, an institutional subscription cost $12 per year. In 1962 *Chemical Abstracts* published 165,000 abstracts and the 1963 price will be $500 to American Chemical Society members and to colleges and universities, and $1000 per year to all others (President's Science Advisory Committee, 1963, p. 18).

Some have estimated the total number of papers published annually in the sciences in the early 1960's as over 2 million. In addition to published papers it was estimated that in the United States alone some 100,000 informal government reports are published annually, of which 75,000 are "unclassified" (President's Science Advisory Committee, 1963, p. 19).

This communications explosion within science has given rise to concerted efforts to deal with the problem on the basis of new technological advances. Through the use of computers and a variety of other technical devices, an effort is being made to facilitate the storage and retrieval of information. Considerable progress is being made along these lines, but at the same time questions have been raised about the changing function of scientific communication and a host of other quasi-technical and nontechnical aspects of the communications process. It is in the latter terms that a discussion of communications is relevant within the general topic of the nature of science and its social system.

In his recent book, D. de S. Price (1963) reviewed briefly the history of the scientific paper. He suggested that it came into being originally because there were "too many books." The scientific journal, born around the middle of the seventeenth century, came into being with the function of "digesting the books and doings of the learned all over Europe. Through them the casual reader might inform himself without the network of personal correspondence, private rumor, and browsing in Europe's bookstores, formerly essential" (D. de S. Price, 1963, p. 63). According to Price, the original purpose of these journals was primarily the social one of finding out what was being done and by whom, rather than the scholarly one of publishing new knowledge (1963, p. 63). Price, relying heavily on Barber's (1961) paper, went on to state, "original publication of short papers by single authors was a distinct innovation in the life of science, and like all innovations it met with considerable resistance from scientists" (D. de S. Price, 1963, p. 63). It was not until about the middle of the nineteenth century that the short paper as an independent unit began to appear. In addition to communicating new knowledge, one of the prime factors in the establishment of the scientific paper as the mode of communication is the necessity to maintain and establish one's intellectual property—as Derek Price put it, "the never gentle art of establishing of priority claims" (D. de S. Price, 1963, p. 65; cf. Merton, 1957a; Merton, 1961; Merton, 1963).

Publications have assumed still another function which has played an increasingly important role in modern times. The number of publications a man has produced is generally accepted (despite the usual reservations) as a measure of a man's scientific worth. The fact that this is so has "moved people to publish merely because this is how they may be judged" (D. de S. people to publish merely because this Price, 1963, p. 40). Until the 1950's or so these judgments were primarily those of deans, chairmen of departments, research directors, and the like, in evaluating a man's promotion or salary increase within his own institution (see, for example, Caplow & McGee, 1958). Recently, however, with the growth of the project grant system and the expansion of federal aid for research, it has become almost as necessary to publish papers simply to continue to receive research support, irrespective of whether one is in line for promotion or salary increase at that time.

In a development related to the changing social organization of research, the number of multiple-authored papers has risen sharply since the beginning of this century. Moreover, in recent years it has been possible to find articles with as many as a dozen authors listed. Some (e.g., D. de S. Price, 1963, p. 90) see the beginning of a new trend wherein none of the authors is listed; instead the name of the research team or organization is listed as the author of the scientific communication.

One of the best examples of pioneering social research in the science communications area is the series of studies conducted at the Bureau of Applied Social Research (1958; 1960). In an exploratory study at a single university, Mentzel and his associates conducted intensive interviews with 77 scientists in biochemistry, chemistry, and zoology to learn about their communications behavior. They were interested primarily in the following kinds of questions: (1) What are the scientist's communications channels for exchanging and gather information? (2) What are the varying functions of scientific communication? (3) What are communications' "needs" and how well are these satisfied? (4) What are the situations in which the exchange of information takes place? (5) What are the conditions and opportunities which influence information needs and information-gathering habits?

On the basis of this exploratory study, Menzel (Bureau of Applied Social Research, 1958, p. 132) raised a number of important research questions. In particular, he pointed to the range of functions, both manifest and latent, as well as the range of possible means, of communications. He noted the importance of a variety of informal channels, some of which D. de S. Price (1963, Ch. 3) later aptly labeled the "invisible colleges."

Since that study Menzel (Bureau of Applied Social Research, 1960) has also published a review of related studies and has been concerned with a series of other studies which would build on the initial exploratory work already completed. Parenthetically, it might be noted that this work on the communications behavior among scientists can be traced back fairly directly to some of the early communications studies done at the Bureau in the 1940's and 1950's. In particular there seems to be a direct line from the Decatur study (Katz, 1957; Katz & Lazarsfeld, 1955) to the more recent and somewhat more closely related studies of diffusion of knowledge among physicians, especially concerning new drugs (Coleman, Katz & Menzel, 1957; Coleman, Menzel & Katz, 1959; Menzel, 1957; Menzel, 1960). In this, and in subsequent studies undertaken at the Bureau, the scientific communications process is viewed as part of a larger social system. While professional information experts, librarians, editors, abstractors, and others seek a variety of ways to improve communications by electronic and other devices, sociologists are making, and will make, their contribution by pointing to the network of social relationships in which communication is embedded and to the inadequacy of restricting attention solely to formal means of communication.

As Menzel (Bureau of Applied Social Research, 1958) pointed out, and as others have increasingly begun to recognize, face-to-face and interpersonal communication plays a role of ever-increasing importance in scientific communication. As the number of scientists has increased and specialization has been intensified and as the time lag between the publication of a paper and its submission for consideration has increased, so the need for alternative modes of communication has arisen. In this situation it is less than surprising that interpersonal communications have come to the fore. In addition there has arisen what is now institutionalized as the "preprint" (the mimeographed or dittoed document) which is privately circulated. As one observer recently put it, "with respect to preprints, science faces a real danger of reverting to the privacy of the 17th century; some biologists think this has already happened to molecular biology, where preprints are often circulated only to one's friends" (Weinberg, 1963, pp. 68, 71). This quotation is from an article which summarized a part of a recent report of the President's Advisory Science Committee, chaired by Dr. Weinberg, on "Science, Government, and information." As this report noted,

"Transfer of information is an inseparable part of research and development. All those concerned with research and development . . . must accept responsibility for the transfer of information in the same degree and spirit that they accept responsibility for research and development itself.

"The later steps in the information transfer process, such as retrieval, are strongly affected by the attitudes and practices of the originators of scientific information. The working scientist must therefore share many of the burdens which have traditionally been carried by the professional documentalist. The technical community generally must devote a larger share than heretofore of its time and resources to the discriminating management of the ever increasing technical record. Doing less will lead to fragmented and ineffective science and technology" (President's Science Advisory Committee, 1963, p. 1).

Not only does the report urge the technical community to recognize the importance of handling information adequately, stressing that it is an integral part of the scientific process (one wonders why it should be so necessary to stress this), but it also urges that new techniques and methods be explored for what it calls "switching," by which is meant devices for connecting the user with the information (as contrasted with the documents) he needs. Among the suggested methods are: specialized information centers; central depositories; mechanized information processing; and the development of what is termed "software," which indicates the panel's recognition that hardware alone is inadequate for coping with the problems of information retrieval. In the panel's view, software includes methods of analyzing, indexing, and programing for successful information retrieval. Although Weinberg (1963), a nuclear physicist by training, and the other physical scientists on the panel are undoubtedly aware of some of the social aspects of this communication process, it is regrettable that so little of this shows in their report and recommendations.

While it can be said that a start has been made toward the study of many aspects of the scientific communications process, almost nothing has been done about yet another aspect, namely, the scientific convention or meeting. Sociologists themselves know about it primarily because they are participants in this process and are likely to hear jokes, sarcastic remarks about "living it up," and other such informal characterizations of the changing nature of scientific conventions. For example, the following item appeared in *The Observer:*

> Dr. William H. Pickering, the president of the American Rocket Society, says that the space industry spends, directly and indirectly, 150 million dollars a year on attending and exhibiting at technical conferences. . . . The same technical papers, thinly disguised, are presented again and again . . . (and) many people spend most of their time shuffling from one conference to another (*The Observer*, November 5, 1961, p. 4).

One specialist told of an international scientific conference at which some seven hundred papers were "read" by title only. In other words, there was no communication of scientific findings at all (except what might be gleaned from a brief title). For many scientists, however, international congresses and other large gatherings are not as valuable only for the formal papers as they are for the opportunity to interact face-to-face with a number of fellow specialists. Except for anecdotal reports, then, there have been no studies of the changing functions of scientific congresses, as well as of the different kinds of scientific meetings which have sprung up.

The communications area has been treated in some detail to provide examples of the kinds of research questions which may emerge from considering one important facet of the social system of science. The field of scientific communications is an interesting case where applied interests and new technical developments spur research efforts from which may come new basic knowledge. As more social scientists delve into these problems, it is highly likely that more of the total social system of science will be opened up to fruitful inquiry. One such direction of obvious importance is the individual scientist as a subject for further study.

THE NATURE OF THE SCIENTIST

Shortly after World War II, it was estimated that altogether there were 140,000 people engaged in science in the United States (*Fortune*, 1948). Of these only some 25,000 held the Ph.D. in one of the natural or physical sciences. By 1960 it was estimated that there were 1.4 million scientists, engineers, and teachers of science of whom 87,000 had a doctorate (National Science Foundation, 1961). Who are these people? What is known about this increasingly important yet tiny fraction of the population?

Strangely enough, little is known today about the social characteristics of American scientists. It is highly unlikely that a study of America's scientists in the mid-1960's would find (as the post World War II studies found [*Fortune*, 1948; Steelman, 1947, Vol. III, Appendix III]) that a large proportion of biologists came from a rural farm background, while physical scientists were more likely to have come from a middle-class, Protestant, small-town or urban background. The social background of today's scientists is different not only because of changing population patterns within the country, but also because of the broadening of the recruitment base for many of the sciences. It seems strange that so little is known because this is probably the one area in which sociologists have the greatest immediate capability in conceptual and methodological tools. This is especially true for the sociologists of occupations and professions.

The Creative Scientist

Much of the existing knowledge about the nature of scientists derives from a small number of studies by psychologists interested in the creative scientist. Perhaps the most widely known, most influential, and most frequently cited one is the study by Anne Roe (1953). Although she chose a total of 64 outstanding scientists in 4 different fields and made no attempt to generalize to all scientists, her work is frequently cited in support of various statements made about scientists in general.

McClelland (1962) listed the following characteristics of physical scientists that seem to him to have been confirmed in various studies, recognizing the difficulty and danger of making generalizations based on the varied and small populations studied.

One way of minimizing the difficulty is to try to select only those characteristics which are so striking that they apply (with variations, of course) to all scientifically oriented subjects but in greater degree to those who are more creative or eminent. Another way of minimizing the danger of too-sweeping generalizations is to focus on experimental physical science—in particular on physics and chemistry. Theoretical physics and mathematics shade off in one direction from such a focus and the biological sciences in another so that any statements made need not apply as fully to scientists in these areas. With these guidelines in mind the following generalizations would appear to summarize fairly well the characteristics of physical scientists as they have been uncovered by investigations up to the present (McClelland, 1962, pp. 143–144).

McClelland's (1962) generalizations are as follows: (1) Men are more likely to be creative scientists than women. (2) Experimental physical scientists come from a background of radical protestantism more often than would be expected by chance, but are not themselves religious. (3) Scientists avoid interpersonal contact. (4) Creative scientists are unusually hard working to the extent of appearing almost obsessed with their work. (5) Scientists avoid and are disturbed by complex human emotions, perhaps particularly by interpersonal aggression. Scientists react emotionally to human feelings and try to avoid them. (6) Physical scientists like music and dislike art and poetry. (7) Physical scientists are intensely masculine. (8) Physical scientists develop a strong interest in analysis in the structure of things, early in life (McClelland, 1962, pp. 144ff).

In a study of 40 scientists, Eiduson (1962) suggested the following list of personality characteristics:

(a) The scientist has strong emotional leanings to intellectual activities; (b) he is independent in his thoughts and actions and does not mimic others; (c) he is challenged by frustration and anxiety-producing situations; (d) curiosity is likely to be the major determinate in his work; (e) strong ego involvement and conflict are expressed in work; (f) he does not use parental ideals to set up his own goals; (g) he shows a strong capacity for sensual gratification; (h) he is motivated by a desire to master or interpret natural forces or reality; (i) he is sensitive to the moods and feelings of others; (j) he is sensitive to his internal environment, needs, wishes, desires; (k) he values his work primarily as permitting expression of inner personality (Eiduson, 1962, pp. 86–87).

Such lists could be multiplied almost endlessly. The interested reader is referred to two major sources in addition to the ones already mentioned: Taylor and Barron (1963), which contains selected papers from the first three Utah Conferences on the Identification of Creative Talent; and Stein and Heinze (1960), which is a detailed annotated bibliography of the more important works in this field.

Fascinating and important as the study of creativity may be, a detailed review, especially of the psychological and psychiatric studies, would be out of place here. This subject will be returned to again in the next section, where some of the studies of organizational and other social factors which may influence the creativity of scientists are reviewed.

Many of the psychologically-oriented creativity studies were designed to ascertain the characteristics of creative scientists in order to devise effective selection procedures for potential recruits to science. This follows the traditional psychological strategy of determining attributes of "successful" people and designing standardized tests which attempt to tap those particular attributes. The reader now comes to the more general area of the recognition, recruitment, and selection of scientists.

Selection and Recruitment of Scientists

Perhaps the best recent summary of the state of available knowledge about the characteristics of the scientist in general is contained in a volume by Super and Bachrach (1957). The aim of this review was to summarize what was already known about scientific careers, the characteristics of the natural and physical scientist, the mathematician, and the engineer, together with recommendations about further research which seemed advisable.

As the authors of this volume noted: "The portrait of the successful natural scientist which emerges from the general literature is that of a paragon" (Super & Bachrach, 1957, p. 1). According to these studies the scientist is capable of rigorous and abstract thinking and of a high level of achievment; he has good verbal reasoning ability, a high level of reading speed and comprehension, an extensive vocabulary, a facility of expression; he has superior scholarship, superior quantitative aptitudes, good spatial visualization, high mechanical comprehension, superior manual dexterity. He is also said to be ingenious, curious, industrious, full of initiative, strongly inner directed, enthusiastic, energetic, exceptionally honest, imaginative, and he possesses originality and high analytic ability. The authors state that "the stereotype of the scientist as a lonely, socially inadequate, and somewhat withdrawn individual, curious, self-disciplined, unemotional, tolerant of others, and intensively devoted to his work finds considerable support in the research literature" (Super & Bachrach, 1957, p. 3). The potential scientist is likely to become interested in science rather early in life, often around the age of 10. He comes from an upwardly mobile middle-class family background which can be characterized as intellectually stimulating and well endowed."

But the authors noted in their conclusions: "Research has been based largely on trait-and-factor theory, derived from the psychology of individual differences and from a static approach to social factors" (Super & Bachrach, 1957, p. 6). A large proportion of the studies reviewed are concerned with success in college, with the main criterion often that of success in college courses. The authors stated: "There is an overemphasis on intellectual factors and other easily measured characteristics, but there are too few studies investigating such less easily assessed and quantified factors as personality traits and motivation" (Super & Bachrach, 1957, p. 7). One particularly promising exception to this kind of approach may be found in Cooley (1958). Many of the studies are relatively static in design instead of longitudinal and rarely use carefully selected samples of chemists, biologists, and the like, as opposed to relatively heterogeneous samples of "scientists."

Fortune magazine (1948) reported on one of the first nationwide sample surveys of American scientists and continued its pioneering ways with a series of articles in 1960 on the "Great American Scientists" (Editors of *Fortune*, 1961). While not adding much to our systematic knowledge of scientists in general, these perceptive articles brought together the personal life histories of some of the great scientists, together with a discussion of the work they had accomplished in the development of a number of specific scientific fields.

Some information about scientists as well as about the academic institutions which produced them is to be found in the classic studies by Knapp and Goodrich (1952) and by Knapp and J. J. Greenbaum (1953). They showed that the smaller, liberal arts, middle- and far-western colleges were much more effective producers of scientists than other academic institutions. This so-called institutional productivity hypothesis was reexamined in the light of certain studies connected with the National Merit Scholarship program. It was suggested that "institutional productivity" was "a function of the differential college attendance, paternal vocational motivations, and their implied correlates among high aptitude students" (Holland, 1957, p. 437).

Some notions of the characteristics of the American scientist have been derived especially from studies among high school and college students of their images of the scientist. Mention should be made in particular of the study by Margaret Mead and Rhoda Metraux (1957) and the study by David C. Beardslee and Donald D. O'Dowd (1961). However important these images are for the recruitment of new scientists, they obviously tell much more about images than about the actual characteristics of scientists in America today.

Because of limitations of space it is necessary to omit a number of manpower and recruitment studies, as well as studies in a number of related areas (e.g. Brown & Harbison, 1957). Many of these have grown out of the pre-Sputnik concern with the shortage of engineers and the post-Sputnik concern with the shortage of scientists generally. In addition, there have been a number of studies which have attempted to add to the knowledge of the early recognition of potential scientists, selection procedures, aptitudes and motivations, and measures of potential ability.

In summary, little is known of the sociological aspects of the nature of the scientist in America today. As noted earlier, there have been no nationwide surveys of scientists since 1947–1948. The *Reader in the Sociology of Science* by Barber and Hirsch (1962), for example, does not have a single selection on the current characteristics of scientists. It does have selections from some of the sources mentioned earlier on high school and college student images of scientists, as well as the Knapp and related studies. There have been several Ph.D. dissertations, for example, Krohn (1960; 1962) and Merz (1961) which, although confined to relatively local and small samples, suggest that there have been enormous changes in the characteristics, values, and attitudes of the men being recruited into science today. Until new comprehensive studies are completed, knowledge of such matters remains unsatisfactory.

THE ORGANIZATION OF SCIENCE

There are still some scientists among the 4 million or so in the world who work alone at the bench. There are no accurate figures, but it is highly probable that few of the world's scientists now do so. Even where they are directing their own projects, they are likely to have a number of collaborators and assistants. And even the minority who work alone are likely to be part of an organization devoted to research.

Recently, sociologists have become increasingly interested in studying a variety of large-scale organizations (Barton, 1961; Etzioni, 1961). But few of these studies, as will be seen below, have been concerned with scientific research organization, whether in universities, government, or industry.

The first part of this section will discuss the internal organization of research; the second part will focus on the external. By "internal" is meant the organization of the laboratory, or of larger units, engaged directly in the conduct of research. Since most such organizations are themselves a part of still larger organizations, the interrelationships of these to each other is included. Conceptually, these "parent" organizations (a university, an industrial company, a government agency) might well be treated as part of the external system (Kaplan, 1959a; Vollmer, 1962). So little has been done to develop this potentially promising distinction, however, that most of the discussion of this topic is included in the first section.

The external organization emphasized in the second part is the larger national context in which research activities are carried out. Increasingly, it is the national governments, in the United States and in every other scientifically developed society, which are influencing and supporting the conduct of research. This influence is becoming more direct and more overt and is surely affecting the internal organization of research (Whitney, 1960). To understand why laboratories in a certain country are large or small, permanent or temporary, bureaucratic or not, it is necessary to know something about that country (Kaplan, 1961). To understand whether differences observed result from the internal nature of science or from characteristics of the larger society, more comparative studies are needed. These issues are raised in the latter part of this section. First, however, one should review recent developments in order to learn more about the internal organization of research.

The Organization of Research

The literature on research organization is enormous. The discussion which follows is more of a guide to a few of the varieties available than it is a general review. A recent bibliography of the literature by Rhenman and Svensson (1961), which is labeled as "selected" and confines itself to recent literature, contains nearly four hundred references. One of the first bibliographies on research administration, Bush (1954), contains over 1,100 references, most of them since 1945. There is almost no overlap between these two bibliographies.

With the trend toward increasingly larger and more complex research organizations, a host of problems, many of which are not necessarily inherent in the research process itself, has arisen to interest, as well as plague, scientists and administrators. Scientists and others have been particularly fearful of the effect of what they see as the bureaucratization of the scientist and of scientific research (Speyer, 1957; Tuve, 1959; Whyte, 1956). Organizations have struggled to maintain an environment in which research could be as free as possible, while at the same time imposing what are considered necessary organizational restrictions and regulations. Attempts have been made to standardize criteria both for the effectiveness of research organizations and for its individual scientists (Quinn, 1958; Randle, 1959; Rubenstein, 1957b). The problems calling for study of research organization are myriad (Rubenstein, 1959; Shepard, 1956a). Recent reviews of of particular parts of the literature may be found in Peters (1957), Fogler and Gordon (1962), and Vollmer (1962), to mention just a few sources.

Studies of research organization have followed a wide range of orientations. So, for example, there have been studies essentially in the human relations tradition (Pelz, 1956b; Pelz, Mellinger, & Davis, 1953; Shepherd & Weschler, 1955), the industrial management tradition (Anthony, 1952; Dinsmore, 1958; Hirsch, Milwitt, & Oakes, 1958), the formal-informal organizational tradition (P. Brown, 1954; Marcson, 1960b), morale studies of scientists in relation to supervisory practices (Shepard, 1955), and the interrelationship of professionals on different hierarchial levels within large-scale organizations ((Shepard, 1956c; Shepard, 1958).

One sample of the kinds of problems studied and the results obtained follows. Task-oriented interaction is greater among development groups than it is among research groups (Shepard, Pitkin, Simmons & Moyer, 1954). There is also the suggestion in these studies that the tradition of leaving scientists alone is not as effective as that of encouraging scientists to interact with one another. On the basis of a study of a large government medical research laboratory, Pelz (1956b) and his associates concluded that daily contact with colleagues who do not share one's values leads to better performance. Pelz also suggested, on the basis of the same study, that when the main colleague contact and contact with the supervisor is analyzed, higher performance results when one of these two contacts is with a man in a different field from the respondent.

Many studies have been directed largely to applied and immediate problems, often those set by the organizations being studied. In this vein are the studies concerned with the problem of whether the laboratory should be organized along functional or project lines (Ashcroft, 1959; Pelz, 1956b). There is the problem of the role of administration (Gargiulo, Hannoch, Hertz, & Zang, 1961) and the role of various supporting personnel (Pelz, 1959). Research has been directed not only to ex post facto measures of effectiveness but to trying to anticipate whether one project will succeed over another and whether one will be more profitable than another, regardless of its technical "success." Various attempts have been made to assign financial and other quantitative measures to different choices which might be made (Freeman, 1960; Horowitz, 1960; Johnson & Milton, 1961).

Many of the investigators are former physical scientists who have become research administrators and are thus directly concerned with such problems; some have been operations researchers employed on a permanent or consulting basis; and others have been men with backgrounds in industrial psychology, industrial management, and allied fields, called upon to make studies of this kind in order to answer pressing applied problems. But, increasingly, social scientists have begun to turn their attention to some of these problems, too. What kinds of problems have they attacked? What kinds of results have they achieved?

Some have focused on problems of leadership and supervision in the laboratory. The Michigan studies (Pelz, 1956b), following in a Lewinian tradition, found that "participatory" leadership tends to be more effective than either directed or laissez faire types of leadership. In a more detailed analysis, Baumgartel (1956; 1957) concluded that the leader who is most effective is one who is both technically competent and a good administrator.

Using data from the Michigan studies (Davis, 1956) regarding the National Institutes of Health, scientists were divided into those primarily oriented toward science and those oriented toward the institution. The latter were much more interested in helping people, especially by finding a cure for disease. As it happens, this is also the official goal of the National Institutes of Health. But the Michigan study found that only the science orientation was related to performance as judged by peers. The best performance seemed to occur where there was a high science orientation combined, with a low institutional orientation (Davis, 1956). This suggests, among other things, that the ratings by peers were based on the values of general science, rather than medical values which might stress the cure, of patients. For another example of the effect of scientists' orientations on research in an agricultural research setting, see Storer (1961; 1962a).

One of the first full-scale published case studies of a research and development organization was by Marcson (1960b). The central theme of this work is the conflict between traditional business ideology, organization, and concepts of authority, and the traditional values of the scientist.

Marcson (1960b) made a contribution in his careful description and analysis of both the formal and informal organization of the laboratory. This is scarce in the literature, and even where it does appear there is little appreciation of the subtleties of the interrelationship between the formal and informal, such as are treated in the Marcson volume (see especially Ch. 3). The scientist finds himself typically in the work group rather than alone, and these work groups, according to Marcson, become informal networks of interpersonal loyalties which not only help in the conduct of research but, presumably, satisfy some of the scientist's demands for interpersonal recognition and colleague relationships.

These many suggestive observations and interpretations point up how much there is yet to learn about scientists and their research organizations. When single case studies of large industrial or other organizations first began to appear in sizeable numbers in the sociological literature, a clamor sounded for something more. Valuable and necessary as these are at the outset of an attack on the problem, they emphasize the need to go on to comparative studies in which many more precisely defined variables are systematically examined.

Kornhauser's study (1962) was much more concerned with the general problem of the strains and adaptations between professions (with science as the prime example) and organizations. Although Kornhauser studied six industrial laboratories, a trade association laboratory, a government laboratory, and an independent research institute he made little attempt to compare systematically the different laboratories or to discern the differences among them. One should recognize, he argued, that professionals are increasingly employed by large organizations and hence should not try to rescue the earlier stereotype of the independent professional. Although there is inescapable tension between the values of the professional and the values of the organization, Kornhauser saw the end accommodation as one in which organizations will become more professionally oriented and the professionals more organizationally oriented. Whether one wants to quarrel with this conclusion or not, one of the main difficulties is the undifferentiated nature of both "professional" and "organizational" in this analysis.

Using a somewhat different approach Kaplan (1959b) tried to analyze the formal structure of a research organization through a study of the roles which had been developed. The role of the research administrator was selected as particularly crucial because of its newness and because it did not have an exact counterpart in other types of organizations. The research administrator was seen as a role in which the conflicting policies and demands of the organization on the one hand, and the scientists, on the other, were focused. The growing importance of this role in research laboratories in the United States seemed to indicate the increasing bureaucratization of research laboratory organization. But, in a companion piece on the role of the research administrator in Soviet medical research organization, Kaplan (1961) suggested that increasing bureaucratization is not a necessary result or by-product of the creation of such a role.

In almost all cases the dependent variable, or the factor which most investigators sought to explain, was labeled as productivity or performance. The studies reviewed here suggest that there are in fact many organizational, social, and other "nontechnical" factors which influence scientific productivity, but the results are still ambiguous or contradictory. Moreover, there is still considerable work to be done with the dependent variable. So far, investigators have relied on the following indicators of scientific productivity or performance: (1) the number of papers published or the number of patents issued (Ben-David, 1960b; Lehman, 1954; D. de S. Price, 1963); (2) the number of citations received by a paper as an index to the quality of the paper, on the assumption that the higher the quality the more often it will be cited in other people's works (Comrey, 1956; Garfield, 1955; Meltzer, 1956; Platz & Blakeloch, 1960), this latter standard being introduced because of the general discomfiture with simply counting the number of publications and equating, in effect, the paper by Einstein in which he developed his theory of relativity and the one by Mr. Jones reporting a replication of a trivial experiment; (3) other ratings of productivity—such as self-ratings, ratings by one's peers, ratings by a specially selected group of seniors in one's organization, ratings by immediate supervisor, ratings by middle-level supervisors, and ratings by the chief of the research organization (Pelz, 1956a; Pelz, 1960; Pelz & Andrews, 1961; Pelz & Andrews, 1962).

Some relationships have been found with all of these methods of determining scientific productivity. No relationships have been consistently strong in one direction or the other. Nor is any investigator entirely happy with any of these measures. Clearly, the development of new concepts and techniques to measure research productivity will aid the study of research considerably. One intriguing hypothesis, which Pelz (1960) suggested, is that when there are factors in the scientist's social or job environment which jostle him intellectually, his performance becomes higher. This jostling or uncertainty was labeled "dither" (following Weaver's [1959] use of this term). The general notion was that uncertainty and anxiety go together in promoting creativity and high performance for a scientist, but each of these factors should be operating in opposite directions at any given time. Thus, when uncertainty is high, anxiety should be low. Pelz (1963) now seems to feel that an atmosphere of intellectual "dither" is more functional for the scientist engaged in research, while experience or cumulative wisdom is more valuable for the man in development work.

Despite the apparent lack of a common framework, two general problems stand out. The first is concerned with the effects of the research atmosphere or environment on scientific performance. The second might be put roughly in reverse terms: The effects of expanded research activities on the traditional nature of the parent organization.

Both problems make sense theoretically, but neither has stemmed primarily from theoretical concerns. It is largely because these have been applied, policy-seeking research undertakings that there has been a peculiar balance in the resulting empirical work. Thus, studies of the effects of the research environment have been concentrated in industrial laboratories, occur to a lesser extent in government laboratories, and are least likely to be found in university laboratories. The reverse holds for the study of the effects of the research activities on the organization.

While all three institutional sectors have been faced with the problem of how to cope with a considerably expanded, complicated, and much more expensive research operation than had ever been known, each sector chose to focus on a different set of problems. One of the best recent series of descriptive papers emphasizing the role of basic research in each of the different institutional sectors may be found in Wolfle (1959b). In part, this arose from a set of untested assumptions. The first holds that the university is really the "natural" home for research. The second holds that research is a set of activities sufficiently different from all others performed in the organization (especially in industry), that it must be differently organized, or, at the very least, that there are special problems in trying to organize research. The suitability of the academic model to some research organizations has been questioned, and the so-called project form of organization has been substituted in whole or in part. Herbert A. Shepard (1957) has been among those social scientists who have devoted considerable attention to this particular problem. The first two assumptions are often compounded with a third, namely, that scientists are essentially different from most other employees and consequently have to be "handled" differently. Randle (1959) is one of a number of industrial consultants beginning to question these assumptions.

In part, the different emphases have arisen from certain inherent differences in orientation characteristic of each sector. In the era of the cost accountant in industry, each activity, each department of the company, must not only pay its own way, but show a profit, and one of the main problems for industrial research was to devise ways of attaching costs and profits to the research operation (National Association of Cost Accountants, 1955; Rubenstein, 1957a). But it was generally recognized that other means had to be devised (quantitative if possible) to evaluate the effectiveness of the research program and operation. This gave rise to a variety of approaches to the study of the problems of appraising research.

In industry, the search has been concentrated on devising a formula, or some other precise indicators, which would help management decide on which of a number of proposed projects should be selected and how to evaluate progress of on-going projects. For a general review of some of the problems involved, see Rubenstein (1957b); for a review of some of the actual approaches tried, see Rubenstein (1957a).

Neither government nor the universities seem to have been as concerned with these sorts of appraisals. This is probably not the case for a considerable part of the government's military research effort, but practically nothing is available for study because of security restrictions.

Still another concern of some investigators has been the relationship between the organizational environment or atmosphere and the yearning for creative (as well as productive) research. Stein (1955; 1959) conducted an elaborate series of studies which seek to explore the interaction of purely psychological characteristics with organizational and general environmental factors. A series of volumes will report the final results. Stein is one of the leading exceptions to the generalization that most of the psychological studies of creativity have disregarded organizational environment (Taylor & Barron, 1963). Some of the factors which have been overlooked in these studies are discussed by Kaplan (1960b; 1963a), in which an effort is made to specify factors in the different institutional environments in which research is conducted.

Although there are a number of studies with the words organizational atmosphere and organizational environment in the title, very few treat the matter in a genuinely analytical way (Orth, 1959). For example, Pelz and Andrews (1962) relied primarily on the goals of the laboratory, and dominance by Ph.D. scientists, as the distinguishing characteristics of atmosphere. Gordon, Marquis, and Anderson (1962) considered the possible range of freedom and control and their combinations in different settings. Since it is obvious that there are some industrial laboratories (e.g., Bell Laboratories) where there may be considerably less restriction on the scientist's freedom than may be found even in some university organizations, Gordon and his colleagues advanced the hypothesis that it is the immediacy and specificity of research goals rather than the institutional context which varies directly with the controls exerted on the scientist, although there tends to be some relationship between these two factors.

During the early 1950's the prime concern of most educational institutions centered on problems of financing research (The Committee on Institutional Research Policy, 1951). Kidd (1959a) was the first to publish a full-scale study of the changing interrelationships of the federal government and the universities as a consequence of increased federal support for research. Kidd was especially concerned with the effects (at that time largely unrecognized by the academic community or, more precisely, the academic hierarchy) of large-scale university research on teaching and on the other goals and practices of the university.

Since that time there have been an increasing number of studies of various facets of this new problem. Rivlin (1961) analyzed the developing financial relationships between the federal government and the universities and raised questions about outright subsidization of higher education and research. Orlans (1962) studied 36 institutions of higher learning to assess the impacts of federal support on the quality of education, the organization and administration of the universities, and a host of other factors. The Carnegie Foundation for the Advancement of Teaching (The Carnegie Report, 1963) sponsored "self-studies" at 26 selected campuses, centering inquiry on many of the same problems as the Orleans study. The entire issue in which this report appears is devoted to "Partners in Search of Policies: Higher Education and Federal Government."

Finances continue to occupy the attention of many educators, although there seems to be less overt fear of federal "control" as a concomitant of federal support (Kaplan, 1960a).

These studies indicate that the major research orientation still tends to be applied and practical rather than theoretically directed. One of the best examples of the kind of research needed may be found in a series of articles by Ben-David (1960a; 1960b; Ben-David & Zloczower, 1962). Although these have been concerned mainly with the nineteenth century there is every reason to believe that the same sort of sophisticated sociological analysis of the contemporary situation is possible. It would be necesary to take into account not only the structural and other features of the universities themselves but, also, and more important, the value system which guides them, the larger social systems in which they operate, and the interaction of various parts of the social system (Kaplan, 1961). So far, the purely practical studies have not succeeded in casting such a wide net.

National Organization of Science

Aside from the specific interest in the role of the larger environment as a factor in the internal organization of research, there is a growing interest in the total organization of science within any given society. Until recently such interests were manifested primarily in historical studies, for example in Dupree (1957) and in Cardwell (1957). But it has only been recently that the overall organization of science has been viewed as a significant contemporary problem.

Up to World War II science continued to expand mainly as a result of a series of individual decisions made in the universities and in other laboratories and by individuals attracted to pursue science. Competition within a particular nation and among the several nations served to correct major imbalances in the total scientific effort, although not always effectively or successfully (Ben-David, 1960a; Ben-David, 1960b). This was in many senses a much better example of a true system of laissez faire than the economic system ever was.

The failure of the laissez faire system can be seen in the shift of leadership in scientific pursuits from Europe to other countries, chiefly the United States. The rapid rise in the number of Nobel prizes won by scientists who are residents of the United States is but one of the many such indices. The increasing emigration of European scientists to America is another (Dedijer, 1961; National Science Foundation, 1962). Some of the factors involved in the decline of European science are also apparent from such recent papers as those by Renée Fox (1962), Consolazio (1961), and Kaplan (1962). The underlying theme of these papers might be summed up as a concern with the manifest inadequacies of the old system, concentrated in, but not confined to, the university, to cope with the development and promotion of modern science in the world of today.

One of the best indices of the failure of the laissez faire system for science lies in the scope and amount of effort currently being exerted to reorganize and revitalize the organization of research in much of Western Europe. Accompanying this reorganization is the recognition, for the first time in many instances, that almost no systematic information and data on the organization of science at the national level existed previously. Social scientists in Europe, no less than in the United States, have remained unaware of the rapidly changing nature of the organization of science and have laid little of the groundwork necessary to enable the practitioners to make decisions concerning the various facets of the total organization. To cope with this gap in information a number of *ad hoc* studies have been instituted by specially created commissions of scientists and government officials and by other national agencies. But almost everywhere there was explicit recognition of the necessity for systematic data on the state of the present system before proceeding to recommend changes and modifications.

In recent years the American Association for the Advancement of Science sponsored a symposium which resulted in a series of papers on the overall organization of science in Great Britain by Hiscocks (1959), Major (1959) on Norway, Ballard (1959) on Canada, and Don K. Price (1959) on a comparative summary. Korol (1957), DeWitt (1960; 1961a; 1961b; 1961c; 1962a; 1962b), and Vucinich (1956) all contributed studies of the organization of various aspects of the Soviet research organization. Orleans (1961), Lindbeck (1961), and Thompson (1963) studied the scientific research organization of mainland China. There is also an excellent short study, by a Belgian sociologist (Molitor, 1960), of the United States national organization of science, done under a Ford grant while he was still Secretary General of the Belgian National Research Council. Finally, there are a number of studies by a variety of government agencies, as well as international ones such as the Office of European Economic Co-operation (1954; 1960a; 1960b), and in the last year or so by the International Office of the National Science Foundation (Watson, 1962).

Despite the inadequacies of many of these studies and the variability in the local situations, a number of common themes are readily evident. They may be summarized as a concern with the complexity of the existing organizational arrangements. This includes arrangements for financing research, for conducting research, for promoting specific programs of research, and for advising on new directions which scientific research should take. Coupled with this is the obvious fact that science is no longer a small, self-contained, autonomous, self-governing community. Although each of these points cannot be treated in any detail in this chapter, it should be pointed out that the sheer change in the size of the scientific endeavor is bound to have ramifications far beyond the increase in the number of people involved. The role of scientists in exerting some control over their efforts and products is also changing as a result. Whether the new scientific establishment which emerges in the years ahead can ever maintain some semblance of self-government is an open question. Some of the lines being followed in an effort to answer this question are discussed in the next section.

SCIENCE AND SOCIETY

The new scientific establishment is much more intimately related to society than ever before. In fact, one could ask whether it is still possible, except for theoretical purposes, to speak of modern science without society, or of a modern society without science. Certainly, the recent studies reviewed in this section attest to the growing interdependence of science and society.

The impact of science on society has always been far better developed and explored than the impact of society on science (Merton, 1952). Today this distinction is quite blurred; American society is a scientific society. One used to conceive of the impact of science on society in much the same way as one would regard Newtonian forces—where an external force exerts pressure on an object and causes it to move in a certain way. The topic is no longer clearly restricted to its effects on employment and unemployment, or on new technological developments (Waterman, 1962). Science is making it possible either to revolutionize or to destroy society as it has been known, and the choice is now largely a matter of social and political arrangements.

In the last decade or two the interrelations of science and society have become more intimate, overt, and direct. For example, the "market place of ideas" as a major mechanism determining choice of scientific problems is rapidly being replaced by a deliberate attempt to link the goals of society with the research goals of science. Scientists are not forced to work in these socially approved fields; they may still disregard these and work on problems of their own choosing, no matter how "irrelevant" the society may consider them. There is no question, however, that it is generally easier to obtain financial support and facilities, and especially to obtain a more adequate share of these, if one chooses to work in the areas defined as socially desirable. For a most perceptive discussion bearing on some of the newly developing patterns for doing research, the work of a physicist, Holton (1962), is especially important.

The "traditional autonomy" of science is being modified from yet another direction. Political, military, economic, and social policies have become so intertwined with, and dependent upon, science that scientists are increasingly being called upon to act as advisors upon political matters which often have some technical aspects.

Conceptually, it is simple to distinguish between two types of science advisors: one is concerned with what happens inside the world of science, while the other is supposed to bring his scientific knowledge to bear on political and other types of nonscientific questions. But, practically, it is difficult to separate these roles (Lang, 1963; Sayre, 1961).

Wohlstetter (1962), a RAND analyst, has criticized a number of physicists for their role during the fallout and testing controversy in the late 1950's. Wohlstetter took the scientists to task for speaking out as "experts" on subjects which he defined as essentially nonscientific and political. Since equally prominent and respected scientists were on opposite sides of many of these political issues and since both sides seemed to rely on scientific data, the problem of which scientist to believe, and on what basis, was magnified.

A recent study by a political scientist, Gilpin (1962a), analyzed the changing role of the scientist as political advisor and as political activist since the end of World War II. Each of these roles is new for most American scientists, and there are undoubtedly many scientists who still wish they could stay out of politics. In Gilpin's view the end of political innocence for many scientists came with the Oppenheimer case (Gilpin, 1962a). Whether or not one dates the loss of innocence then, Gilpin's review of the scientist's role in shaping early United States' policy on nuclear weapons, the control of atomic energy, the development of the H-bomb, through many other military-political decisions of the cold war decade, must be contrasted with the United States scientist's political innocence prior to the war. These problems deserve more intensive study than they have so far received.

Wohlstetter's (1962) open attack on the scientists as political advisors may perhaps be viewed as an index of the early stages of the institutionalization of this new role for scientists. The awesome decisions scientists are being called upon to help formulate, privately and secretly (Snow, 1961), are of concern to all. Fortunately, many of the scientists involved have not taken their responsibilities lightly and have openly discussed (within security restrictions) some of the key issues involved.

In the early years after World War II, the *Bulletin of the Atomic Scientists* provided a forum primarily for the physical scientists. But in more recent years the forum, the participants, and the audience have all expanded. The "Arms Control Issue" of *Daedalus* (1960) was rightly hailed as the best available collection of papers on the subject which had appeared to that date. The decision to develop the H-bomb, as well as many of the other aspects of the nuclear arms race, has gradually been opened to analysis by physical and social scientists alike (see, for example, Dupré & Lakoff, 1962; Gilpin, 1962b; Schilling, 1961; Zuckerman, 1962). As a sign of the times, physical scientists now write about foreign policy not only for other scientists (Kistiakowsky, 1960), but also for foreign policy specialists (Haskins, 1962).

Of special interest in this connection is a conference of both physical and social scientists sponsored by the Council for Atomic Age Studies of Columbia University late in 1962. Some notion of the scope of topics covered can be gleaned from the titles of the papers presented: Gilpin (1962b), *Civil-Scientific Relations in the United States;* Wood (1962), *Scientists and Politics: The Rise of an Apolitical Elite;* Gilpin (1962c), *National Policy and the President's Science Advisors;* Kreidler (1962), *National Science Policy and the President's Science Advisors;* Wohlstetter (1962), *Scientists, Seers and Strategy;* Brodie (1962), *The Scientific Strategists;* and Wright (1962), *The Establishment of Science Affairs.* As a "summary" statement Wright is quoted: "With the benefit of hindsight we now know that we have been living in an age of science for twenty years or more without understanding the implications of this fact" (Wright, 1962, p. 4).

The second type of science advisor is concerned primarily with the internal organization and development of science, insofar as it is possible to separate this from some of the external issues just discussed (Storer, 1962b). The first formal American science advisor (outside of wartime), known officially as the Special Assistant to the President for Science and Technology, had the double duty of "strengthening science" and relating it "more effectively to policy-making" (Killian, 1959a).

Since that time, the President's Science Advisory Committee (1958; 1959; 1960; 1962a) reviewed and made recommendations on a wide variety of problems bearing on recruitment, education, the universities, and the federal government.

Some other sources which should be mentioned in connection with the development of a science policy are Brozen (1962); Price, Dupré, and Gustafson (1960); the text by Dupré and Lakoff (1962); and Don K. Price's (1954) earlier work which anticipated many of these problems by almost a decade. Kidd (1959a), in a first-rate study of the interrelations of the federal government and the universities, and Wolfle (1959a), drawing on his experience as executive officer of the American Association for the Advancement of Science, raised many of the questions which will have to be explored by anyone interested in studying the problems of a developing science policy.

The increasing support for science by Congress and the national government has inevitably affected the relations of scientists to nonscientists. Whether or not one accepts C. P. Snow's (1959) "two cultures" thesis, there is little question of the increasing need for the public and congressmen along with politically influential laymen, to understand something of what is happening within science. Lamson (1960) documented in detail some of the substantial gaps and differences in orientations and attitudes between scientists and congressmen. The need to inform the public on the developments of science has been recognized, and diffusion of knowledge has been accelerated, by such organizations as the National Association of Science Writers. Much more needs to be done to determine how much and what kind of information the public needs in order to inform itself intelligently about scientific developments (Withey, 1959).

Various other steps have been taken and many others have been suggested to strengthen American science. Most of these, characteristically, have involved direct as well as indirect action by the federal government. For example, the suggestion has been made, especially by Senator Humphrey (1960), to create a new Department of Science. This has met with mixed reactions from the scientists (for a summary of some of these, see Dupré & Lakoff, 1962, pp. 69–73; Stover, 1962). Others have argued that most governmental actions have been "relatively minor adjustments in the administrative machinery" and that more basic changes are needed in government, science, and society to cope with the new challenge (Honey, 1960). (An entire issue of *The Annals* [Wengert, 1960] covered a wide range of views on the changing interrelations of science and society—a subject which deserves a more detailed analysis that can be afforded here.)

The tremendous expansion of the physical and natural sciences has had an impact on the development of the social sciences. Although there was controversy, the final version of the National Science Foundation Act did not explicitly prohibit support for the social sciences (Alpert, 1955; Alpert, 1957; Alpert, 1960). A social science program was established quite early, but had a small budget and operated under a cautious set of rules which eliminated many potential applicants. By the early 1960's the social science section was established as a formal division—almost an equal among equals. As significant as the change in National Science Foundation attitude and policy toward the social sciences is, it doubtless reflects a general change in public attitude. The President's Science Advisory Committee, which in 1963 still had no social science representatives, did establish a special panel to study the behavioral science situation. The panel in turn recommended considerably more encouragement for the rapid expansion of the behavioral sciences (President's Science Advisory Committee, 1962b). Undoubtedly the social sciences have benefited some from the "halo effect," surrounding science generally, though in the view of many hardly enough.

The present discussion has touched upon some aspects of newly emerging problems of the advisor for science and of the increasing recognition of the need to formulate public policies for science at the national level (Hailsham, 1963). But already there are indications that such policy-making is unlikely to stop at national borders. The scale of Big Science is such that cooperative research ventures among a number of nations, such as CERN (European Organization for Nuclear Research) for nuclear energy research and the newly formed European Space Research Agency, are essential if smaller nations are to participate at all in certain fields of scientific research.

The Organization for Economic Co-operation and Development, whose work in this area has been mentioned earlier (King, 1962), has had an *ad hoc* advisory group on science policy (1963) which reviewed the possible roles of the OECD (Organization for Economic Co-operation & Development) in promoting science and cooperation on policy among the member nations. Kramish (1963) conducted an extensive comparative analysis of available data on scientific manpower and effort in relation to economic indices in the Common Market countries, the United States, the United Kingdom, and the Soviet Union. Such a report could serve as a basis for an eventual Common Market policy for science, inconceivable as such a step would have been to most observers even a few years ago.

Yet another aspect of the external science policy has been the emergence of various national and international efforts to help promote the growth of science in the newly developing nations of the world. Both the highly industrialized and the industrializing nations have become increasingly aware of the significance of science for accelerating economic productivity.

Ben-David (1962), contrasting the development of science in a new and small nation like Israel with that of the United States, offered some suggestions about the directions which might be followed by smaller and less industrial nations. Stevan Dedijer (1957; 1959; 1962a; 1962b), a physicist turned sociologist of science, has written a number of papers outlining the dimensions of trying to develop science and a science policy in the new nations. Recently there has been an enormous increase of interest among scientists (Blackett, 1962) and statesmen (United Nations, 1963) in the exploration of these problems.

The first international conference devoted to science and the developing nations was held in 1960 at the Weizmann Institute of Sciences in Rehovoth, Israel (Gruber, 1961). Scientists and politicians came together to discuss the role of science—from solar energy to chemical fertilizers to the kinds of physics courses needed in universities of the new states. This was followed early in 1963 by a much more comprehensive conference in Geneva, sponsored by the United Nations, at which over 2,000 papers were presented by scientists and politicians representing 87 nations. The list of papers is in a United Nations (1963) document of 360 pages. The United States contributions were published in 12 volumes, the most relevant in this context being Volume IX (United States, 1963) on scientific planning and policy. For the social scientist, not only the conference papers, but the conference itself is worthy of further study.

This section began by noting how much science and society have become intertwined. In the process, new problems have arisen and new ways of viewing these have become necessary. As has been seen, physical scientists and the whole range of social scientists have become increasingly concerned, as they must, for this new revolution concerns everyone. The natural scientists can hardly speak of the future of science without touching upon the larger implications for man's life span, his health, and well being (DuBos, 1959; Weaver, 1960). It is up to the social scientists to contribute their share toward a greater understanding of these revolutionary implications.

For the scientist, the changes he faces in his way of conducting research pose many questions and problems for the study of research. The effects of Big Science, the changing role of the government, the deliberate attempts to plan and formulate policies for the development of science, and a host of other related changes, have barely been outlined. The changing roles of the scientist outside of the research laboratory, the emerging scientist-statesman and the statesman-scientist have implications which have hardly been touched upon. For the sociologist to ignore these central problems of the time would be a loss for both science and society.

CONCLUSION

The sociology of science is beginning to show signs of rapid development, but it is plainly evident that sociologists have much more to contribute. The majority of works by nonsociologists reviewed here would have benefited greatly from the collaboration of sociologists.

On balance, much has been accomplished recently by scientists whose technical training has been in almost all the sciences. The major progress has been to call attention to the quiet revolution now in progress in and around science and to raise some of the questions which must be asked before answers can be sought.

This review has attempted to raise only some of the questions of all the possible ones which already have been, or still remain to be, asked. The main goal has been to bring forth issues about which sociologists have been less active, and possibly less familiar. The traditional concerns of sociologists with stratification, power, social change, and the other areas covered in this Handbook, are now inextricably a part of the activities of many scientists—those inside the laboratories as well as those outside the laboratories in the capitals of the world.

Sociologists have long argued about the importance of studying social change. Leading sociologists of the nineteenth century, such as Max Weber (1930) did not argue about its importance; they studied change. Science is changing internally even as are views of its earlier developments. The traditional conceptions of the role of scientist as scientist need also to be changed radically. But the new role of the scientist in the forefront of political and economic change implies an even more drastic reappraisal of present views of social processes. The technical expert was always supposed to be "on tap but not on top" and this may still be true of the scientist advisors today. But in the not too distance tomorrow, the scientist may also be called on to be on top, as scientist and statesman become blended into a new role. And where will the sociologists be on coronation day?

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CONFERENCE ON THE ECONOMICS OF MEDICAL RESEARCH

Reprinted from "Report to the President" —— The President's Commission on Heart Disease, Cancer, and Stroke. Volume II, February 1965. By the U.S. DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE —— Public Health Service.

At the request of the President's Commission on Heart Disease, Cancer, and Stroke, Dr. Walter Heller, Chairman of the Council of Economic Advisers to the President, called together a group of economists for a meeting on September 30, 1964, to discuss some of the economic aspects of medical research.

A series of questions served as points of departure for the day's discussion. These were initially submitted by the Commission and elaborated in detail by the Council of Economic Advisers. They are contained in the appendix.

Participants in the conference were:

Economists

Kenneth J. Arrow, Professor of Economics and Statistics, Stanford University.

Peter de Janosi, Program Associate, Ford Foundation.

W. Lee Hansen, Staff, Council of Economic Advisers.

Walter W. Heller, Chairman, Council of Economic Advisers.

Herbert E. Klarman, Associate Professor of Political Economy and Professor of Public Health Administration, The Johns Hopkins University.

Dorothy P. Rice, Medical Economist, Health Economics Branch, Division of Community Health Services, U.S. Public Health Service.

Tibor Scitovsky, Professor of Economics, University of California.

President's Commission

Stephen J. Ackerman, Executive Secretary.

Michael E. DeBakey, Chairman.

Edward Dempsey, Special Assistant to the Secretary (Health and Medical Affairs), U.S. Department of Health, Education, and Welfare.

Mike Gorman, Consultant.

William Kissick, Staff Assistant.

Abraham M. Lilienfeld, Staff Director.

John D. Turner, Staff Associate.

The following is a report of the conference.

The Agenda

The chairman of the Council of Economic Advisers convened the meeting in behalf of the President's Commission on Heart Disease, Cancer, and Stroke. The Commission had asked for some guidelines on what constitutes a reasonable outlay for medical research in general, and particularly research in heart disease, cancer, and stroke. This type of problem is still on the frontiers of economic knowledge and research. Economics has made a fair degree of progress, especially since World War II, in developing criteria for investment in relatively intangible types of activity, such as education. Whether it has progressed to the point that it can provide concrete guidance for medical research expenditures expressed in quantitative terms is questionable.

The concerns of the President's Commission were expressed by its representatives as follows. Medicine has made great inroads in attacking these diseases—heart disease, cancer, and stroke. Even with the knowledge possessed at the present time, medical care can make a great deal more progress. There are many types of heart disease that can be virtually cured today by certain forms of treatment, including surgery. We do not yet know how to affect the course of coronary artery disease. Conceivably there are ways by which some of these diseases may be overcome completely, since the heart performs a mechanical function that can be substituted for by mechanical means. We are able to substitute completely for the heart function in humans for several hours. We can do this in the laboratory on experimental animals for days, with the animals surviving and performing normally in every respect. We cannot do this for longer periods today for mechanical reasons largely. If this problem were solved, the survival level and the productivity of the population would be raised substantially, since heart disease accounts for more than 50 percent of all deaths.

To make the inroads envisioned, funds will be required not only for research, but also for the support of trained manpower, facilities, construction, and so on. The Commission would like to see the health field in as advantageous a position as possible to compete for funds. When a group of diseases costs the Nation large amounts of money, it seems reasonable that they would be assigned a high priority. What is not clear is how one relates the importance of health to other types of national concern and activity, such as defense.

The Commission's representatives also expressed interest in the allocation of resources to patient-care services. This item is additional to the original agenda, which was limited to medical research. Accordingly, they invited discussion of criteria for allocating funds to research, training, and patient care, as well as to research expenditures by disease category. Would it be legitimate to allocate expenditures among diseases according to their respective contributions to mortality? To medical care expenditures? To loss of output?

Another way of stating the Commission's concern is this: Suppose one knew the annual impact on the economy of these three groups of diseases. Assume that medical care expenditures and loss of output due to mortality and morbidity amount to $8 billion annually (as shown by the preliminary data for the year 1962 compiled by the Health Economics Branch of the U.S. Public Health Service). Can economists say how much the Nation can afford to, or should, spend on research and on patient care in these diseases? How does one take into consideration the rising productivity of labor, the growing gross national product (GNP), and so forth?

QUESTION 1: How much can this Nation afford to spend, or how much should it spend, on medical research?

It seems doubtful that a population has any value judgments or feelings about medical research which are independent of its value judgments or feelings about health. If so, the basic question concerning any proposed expenditures in the health field is whether the resulting output will make a commensurate contribution to the health of the population.

Economists hold that it is not meaningful to ask, How much can this (or any) Nation afford to spend on medical research. The real issue is how much it is worthwhile to devote to this purpose in comparison with other purposes, given the expected respective returns from the same resources.

In addition to the size of the disease problem, however that may be measured, there is the uncertainty of return from any research expenditure. It is important to ask whether or not a significant step forward is imminent.

The availability of specialized resources apart, the proper question consists of three elements:

a. What is the estimated value of improving health services—in terms of reduced mortality, reduced morbidity, etc.?

b. What is the probability that this value will be realized through increased expenditures on research?

c. How does this value compare with the estimated cost of achieving it?

It is well to recall that included in the customary calculations of the economic costs of a disease (and the benefits of eliminating or reducing it) are loss of output and medical care expenditures. Other costs, such as pain and grief, are usually neglected because they are difficult to measure.

The economists agreed that the cost of granting relief (public assistance) to support a disabled person, who may constitute a burden to society for a long period, must not be counted separately. That would be double counting, since the loss of his output has already been measured.

Economists have evidently made greater progress in measuring loss of output due to morbidity and premature death than in measuring the value to the individual or his family of avoiding pain and suffering. The question arose whether the attempt to measure the latter does not entail double counting. The answer is that there is double counting if one simply asks how much individuals are willing to spend on medical care, when the anticipated outcome includes the continued or restored ability to work. However, if one carefully tries to ascertain how much individuals are willing to spend beyond any effects on earnings, the danger of double counting is effectively precluded.

The representatives of the Commission pointed out that the greatly increased expenditures on medical research in the postwar period have produced much knowledge that is currently being applied to the care of patients. There has not been a single great advance in medicine in the last 25 years that is not attributable to expenditures on medical research. Without the products of research, physicians would be rendering today the same kinds of service they rendered in the past. In the last 15 years alone many of the congenital forms of heart disease, which formerly were completely hopeless, have become curable. In most instances they led to early death and in a majority of instances to a high rate of disability on the part of those who reached adult life. Today the majority can be cured by surgery. This is entirely the product of medical research.

The Commission's representatives introduced several additional examples. In the past a victim of stroke received a small amount of medical care. His destination and resting place were the nursing home, because medicine did not know what to do for him. Today a majority of all imminent stroke patients can be treated effectively by surgical means.

With present knowledge, early detection of cancer of the cervix would probably yield a 70 percent decline in mortality within a short interval. Rheumatic fever and rheumatic heart disease have become manageable by means of antibiotics, and surgery can be used to treat the resultant heart damage when it occurs.

The economists suggested that efforts be made to quantify the returns from medical research. By definition research is a leap into the unknown, and the only basis for guessing about the probable outcome in the future is what has happened in the past. This is true of all types of research. Only one attempt has been made to estimate the economic return from invest ments in research, namely, in hybrid corn. The yield proved to be of the order of several hundred percent per annum. It seems reasonable to suppose that a research project of modest magnitude would lead to a determination of the return from research that produced the modern treatment of congenital heart disease. Examples in other diseases will readily occur to the investigator. In performing such a calculation, needless to say, one should consider everything, failures as well as successes. It is plausible to assume that too successful a record in medical research ventures—say 50 percent—reflects insufficient expenditures.

The economists agreed that if the procedures developed by medical research prove to be costly to apply, the potential net gain is reduced. This fact will always be reflected in the comparison of expected cost and gain.

The gains due to research are not direct, but are entirely attributable to the effects of services on the health of a population. The provision of services and the effects of services are intervening steps. When the services are not rendered, there are no gains. If the effects of specific services on health are not known, the value of medical research cannot be known. Unfortunately, knowledge of the effect of specific services is often lacking, as for example in coronary heart disease.

The Commission's representatives suggested that since prevention is preferable to cure, research expenditures on preventive measures may deserve priority. To an economist this conclusion is not evident. It is likely that the return from prevention exceeds that from cure, when each is equally attainable. Usually, however, they are not equally attainable. Conceivably the probability of developing effective preventive measures may be low, while the probability of developing helpful curative measures may be high. If so, the latter deserve priority. It is an empirical question. In considering any proposed expenditures on medical research, therefore, the probability of success is an important consideration.

Other things being equal, however, one expects to devote more economic resources to problems of larger size (as measured by the value of the economic loss), because the cost of research is more or less independent of the size of the problem while the value of the return is proportional to it. Presumably, it would cost the same amount to prevent one death through the output of research as to prevent a thousand deaths. The yield of research is a strong example of the operation of economies of scale.

The representatives of the Commission restated the initial question. Given what is known about the size of the economy today and its expected size 5 years from now; given the level of expenditures on medical research; and given the magnitudes of the three disease categories, can economists advise the Commission whether or not the Nation's research endeavor should be expanded?

The economists regretfully expressed their inability to do so. To do so responsibly would require certain kinds of data as a basis for a series of calculations. It is conceivable that very simple calculations will show that increased expenditures are warranted. The size of the economy and the magnitude of the disease problems are elements in the calculation, but other elements enter also. Least important perhaps is the size of GNP, because medical research expenditures are not a large fraction of the total. Obviously the resources exist in this country to permit increased expenditures on medical research. What is important are the probability of success of a given research undertaking and the value of the benefit, if realized. The product of the two factors is the expected value of the research, which must then be related to the cost of this research. Even a small improvement in the probability of success (say 2 percentage points), if it applies to a sufficiently large base, can yield a large gain.

The same is true of a small reduction in the base, especially if the gain is realized in perpetuity. If a given research expenditure could yield an annual reduction in cancer or heart disease deaths of 3 percent, the economic gain would be great. The question is with what probability a reduction can be attained at a given cost.

The economists introduced the concept of time and the discount rate. Both benefits and costs may occur at various times in the future. A dollar today is worth more than a dollar 10 years hence. The discount rate renders these dollars commensurate. The sum of a stream of dollars in the future, properly discounted, is the present value of that stream.

With regard to the initial question, an economist simply would not ask, "Can the Nation afford it?" He would ask, "Is it worthwhile to spend an additional sum on this objective rather than on something else?" To answer this question he will estimate the expected value of a research project (as reflected in diminished mortality and morbidity, and the value attached thereto) and compare it with the cost. If a given amount of money is available for spending by Government, say $100 million, it is necessary to ask whether adding this sum to medical research expenditures would yield better results than adding it to expenditures on education. To the extent that both health and education provide sheer consumer satisfactions, the outcome of the comparison will depend on individual value judgments; to the extent that they both contribute to the GNP, the comparison can be made somewhat more objectively.

The economists offered a reformulation of the "worthwhile" question that may be easier to deal with. A given sum of money spent on research would be equivalent to a 5-percent return on investment if it led to a specified probability of reducing mortality by X and morbidity by Z.

The economists noted that what is always at issue is the marginal or additional dollar, not total expenditures. Everybody would agree that it would be disastrous to abolish all research. It is conceivable, however, that at a given time and place, we may be scraping the bottom of the barrel for problems worth investigating, as well as for talent capable of working on them effectively. Thus, under specified conditions, it may pay to spend $250 million on research, but it may not pay to spend an additional $100 million, or a total of $350 million.

The question was raised whether the criterion for an optimum expenditures policy on medical research could not be something other than the maximum difference between the present values of costs and benefits. Why not adopt a policy of incurring large medical care expenditures, while minimizing indirect costs due to morbidity and mortality in the productive ages? The answer is that the first policy is the correct one, if all the elements are included in the calculation and properly measured, including imputed values for intangible items (see question 4).

The point was also made that even a successful research venture may have a low value ultimately, if the incidence of the disease in question is low. Under these circumstances a screening device of high reliability produced by the research would still yield a high proportion of false positives.

The economists saw no difficulty here. The high costs of rendering effective service with the new screening device enters into the calculation. Direct costs will be high, offsetting the decline in indirect costs. Increased expenditures required to disseminate knowledge also will entail increased direct costs.

A related problem is that the available knowledge is not always applied to reduce mortality and morbidity. There is a gap, then, between the productivity of research and its payoff in practice. It is easier to secure adoption of measures that can be applied on a mass scale than of procedures that require administration to individuals. One of the economists suggested that consideration might be given to calculating benefits in terms of two types of dollar: For example, a 100-cent dollar for research findings that are likely to be applied widely and a 50-cent dollar for findings that are not so likely to be applied.

QUESTION 2: Are there any economic criteria for determining the proper roles of the several levels of government in financing medical research?

In general one would expect a relative shortage of private support for medical research, because the returns from certain types of medical research cannot be captured or appropriated. There is broad agreement among economists concerning this proposition and its corollary, the need of support from public funds—philanthropy or taxes.

Related, though not identical, grounds for support from public funds are the following characteristics of research as an economic good:

1. Its "public goods" aspect: It is an economic good of which the total supply is available to any one individual. "More for you means no less for me." A common example outside the health field is the lighthouse.

2. The presence of external effects: The value of medical research to society or a group is greater than its value to a single individual. In this case reference is made to the furtherance of research through the dissemination of existing knowledge. Another dimension of externality is geographic, namely, that application of the product of medical research cannot be confined within a given geographical area.

3. Economies of scale: Unit cost declines as the volume of output increases. One aspect here is the fact that the reproduction of research is cheaper than the initial cost of producing it. Also, knowledge is cumulative in character and each new building block may be decisive for a breakthrough. Finally, reference has been made to the economies of scale on the benefit side.

4. Uncertainty: The probability of success as an important element in the calculation has been mentioned. Where uncertainty looms large, as in medical research, it pays to diversify risks. This is done by undertaking a large variety of projects, some of which will be successful. Accordingly, total expenditures must be large.

It is reasonable to suppose that unless the returns from expenditures on research are appropriable, private business will not ordinarily incur them. The product of basic research is usually not subject to appropriation. Some products of applied research can be, and are, appropriated as in the case of drugs, which are protected by legal grants of patents for a limited length of time. The dangers here are twofold: (*a*) The guarantee of monopoly provided by a patent may be perpetuated by small changes in the product; and (*b*) a substantial change in the product may not always be compatible with the health of patients. There is some evidence that a proliferation of new drugs has occurred, some of which add nothing useful to the physician's armamentarium.

In summary, one cannot rely on the profit motive to bring about a sufficient flow of private funds into medical research. Therefore, Government must fill the gap, if the public is to obtain the benefits of medical research. Indeed, the question was raised whether as a matter of public policy, Government financing should not be extended into some areas of medical research whose benefits can be privately preempted.

The Commission's representatives observed that although the amount of money spent for medical research by nongovernmental sources has increased, the amount spent by government has increased even more, with the result that the nongovernmental share of the total has declined. There are those who believe that the very increase in expenditures by government will serve to reduce private support. The answer to this argument, it seemed to the Commission's representatives, is that several large philanthropic foundations have undoubtedly moved into other areas of activity, in furtherance of their mission to explore new fields and to develop new programs, leaving to government those activities that no longer represent the new or the experimental. Whatever the reasons may be for the shift in spheres of concentration on the part of the foundations and whatever the justification, the need for funds for medical research exists. If the foundations will not supply the funds, government must.

The economists stated that in this case government necessarily means the Federal Government, for State and local tax funds are unlikely to fill the need. Not only are the States and municipalities short of revenues to discharge all their pressing obligations, but they lack compelling reasons for devoting their resources to medical research. Indeed, the high rate of mobility in the population of this country and the inability to localize the application of medical knowledge means that the benefits of research will not accrue exclusively to the residents of any small area who pay for it. The major exception is an unusually high incidence of a disease in an area or a highly localized disease, as exemplified by air pollution in Los Angeles.

The economists thought that the externality factor associated with geography was so important, that States and local units of government might be reluctant to pay for medical research even if their fiscal capacities were substantially enlarged. The wisdom of the city of New York in appropriating funds for medical research has been questioned.

One economist present believes that financing a major portion of medical research from Federal funds is also justified on grounds of equity. This is akin to the feeling most people have in favor of distributing medical services in relation to need, rather than ability to pay.

The other economists thought that equity is based on broader considerations than the financing of a particular service. Moreover, while people may have strong feelings concerning the ready availability of medical services, regardless of individual ability to pay for them, it seems doubtful that they have any feelings, much less strong ones, on who should pay for medical research.

One of the Commission's representatives observed that medical research and medical services are intimately related, for the provision of services will suffer if medical research does not continue. If considerations of equity enter into the provision and financing of services, they must enter into the financing of medical research. Medical services are merely the delivery of the end product of medical research. Moreover, some of the needed programs of service in heart disease, cancer, and stroke will not get underway, in light of past experience, unless Federal financial support is forthcoming.

On this point there was also disagreement among the Commission's representatives, and no consensus was reached.

QUESTION 3: Are there criteria to guide the allocation of funds between general and specific medical research?

At a meeting at the National Institute of Mental Health in 1963, it was brought out that three of the major advances in the treatment of the mentally ill, which have served to reduce the load in the State mental hospitals, have come from outside the field of psychiatry or psychiatric research. Penicillin has virtually eliminated the late complications of syphilis, including paresis. Vitamin B complex has eliminated pellagra. Tranquilizers were developed in research on cardiovascular diseases. It would seem to an outsider, therefore, that research in specific fields does not always yield the results expected of it.

The Commission's representatives explained the rationale behind the categorical approach to medical research. Individuals have specific, not general, diseases, and medicine deals with specific diseases. Medicine is organized by disease categories (specialties), because this is the most expeditious and most effective way to deal with diseases. This pattern of organization also serves to promote more rigorous research, because concentrating on a specific area of disease tends to sharpen the focus of investigators. Although an important contribution to services in one area may originate in research in another area, most major findings in medicine are the product of categorical research.

There are additional reasons for singling out stroke as a field of research and patient care. In the not distant past, stroke was almost totally neglected, because it seemed a hopeless situation, and few people were conducting research on the disease. There is also the high correlation between the incidence of stroke and age, and our society lacks real interest in the aged. Quite recently new knowledge regarding stroke has emerged as a byproduct of technical advances in radiology. Today one can be optimistic about the returns that will accrue from increased research expenditures on stroke.

Already a great deal can be done to prevent the occurrence of stroke and to help the patient recover from many of its effects. Today the vast majority of stroke patients are still treated by rehabilitation. But recently there has developed the potential of diagnosing impending stroke on the basis of certain signs and symptoms, leading to the application of surgery to prevention. This service is currently available in only a few medical centers, which report excellent results. Two separate groups of physicians deal with these problems, and their facilities are distinct. To stimulate and maintain interest in this disease entity, a categorical approach is required.

The Commission's representatives invited attention to another recent development. In each of the special disease areas, the concept of a multidisciplinary approach (from both the biological and physical sciences) to a particular disease has been widely adopted. Therefore, the new institutes and specialized centers are not nearly so narrow as supposed.

In the discussion of Question 3 the economists were pursuing the implications of a second type of uncertainty, namely, one's lack of knowledge of the disease area in which the returns from medical research expenditures will accrue. Under these circumstances how feasible are calculations on the economic worth of research expenditures in that specific field?

The Commission's representatives replied that there will be no medical progress whatsoever in the absence of expenditures on medical research. Among the issues facing the Commission are: How much more money should be put into medical research in general; how much money should be put into research specifically related to heart disease, cancer, and stroke; and how much money should be put into applying the knowledge gained from research? To be specific, can the fact that 22 percent of all health research expenditures are in the fields of heart disease, cancer, and stroke, while 71 percent of all deaths are attributable to these diseases, serve as a basis for raising the former proportion?

The economists stated that the answer to the specific question is, No. To begin with, the share of research funds allocated to the three disease categories should be based on all categorical research funds, not on total research expenditures. The denominator should be reduced by deleting general research expenditures.

Second, the comparison of research expenditures should be with the economic value of the diseases, not with deaths alone, or the value of deaths alone.

Finally, cognizance must be taken of the probability of success in a particular research undertaking.

Perhaps a more satisfactory and more constructive approach could be taken along the following lines. Suppose applications for research support fall into a number of classes—one general and the others categorical. Why not examine the quality of the applications that are not funded in each class? There may be no objective basis for making such a comparison, but an expert in medical research may be able to state after a review of applications that in his judgment projects being rejected in one class are markedly superior to those rejected in another class. Obviously an economist cannot make this judgment.

The Commission's representatives pointed out that the probability of success in research is not merely a function of the state of knowledge. Psychological and other intangible factors are also involved. Certain areas of investigation attract talent, because Nobel prizes have been awarded in them. Money is sometimes needed to serve as a counterweight, at least initially, in developing an area of research.

In view of the uncertainty concerning the disease area in which the return, if any, from a research expenditure will occur, the general approach to research would appear to the economist to be more logical. Perhaps a review of the evidence would be helpful: How often do returns take place in fields other than those intended?

One can, however, make a case for the categorical approach to research on several grounds. If decisions are difficult to make, one way to facilitate the process is to simplify it and one way to simplify is to establish several arbitrary classes within which all research projects fall. Another consideration is strategic, pertaining to the most effective way to obtain appropriations. Congressmen know about specific diseases. Still another point is that the more categorically oriented a project is, the closer it is likely to be to problem solving than to basic research. A research category should be as broad as the medical specialty within whose purview it falls, the Commission's representatives observed.

The consensus of the economists was that one seems to be dealing here with questions that are largely strategic and administrative in nature. Although economists may have opinions, economics as such has little to say about such decisions.

QUESTION 4: How should one handle certain complicated aspects of the economic calculation, such as the value of pain and grief, the implications of interrelated diseases, and failure to apply new knowledge?

The economists stated that the relief of pain and suffering is truly an economic good to which people, by their actions, ascribe a value. This value in avoiding illness and disability is additional to that of the economic ouput of a person who is well and working. To escape serious illness a person is willing to spend more on medical care than the sum of the averted loss of earnings plus anticipated savings on future medical care expenditures.

There is also a value to human life that goes beyond the survivor's contribution to economic output. It is recognized that it is not easy or simple to measure the value of a human life. However, to avoid the attempt at measurement is in effect to set the value at zero. That is obviously a mistake.

One way to measure the value of pain or grief is to examine what people actually spend in order to prevent disability or to save lives. This provides an indication of their own implicit valuations. The elimination of railroad grade crossings is a good example of a public policy that, in part at least, is based on compassion. Expenditures on grade crossings per human life saved represent an upper bound to the value of compassion.

More explicit valuations are schedules of awards for injuries covered by workmen's compensation, schedules set by insurance companies for other purposes, and verdicts by juries in cases of disability or death caused by accidents.

Under specified circumstances, medical care expenditures actually incurred can be taken as a measure of the value of a certain benefit, which is apart from, and additional to, any prospect of avoiding loss of earnings or of reducing medical care expenditures at a later date. The benefit is that of alleviation of pain when the disease is known to be terminal, as in cancer, and no other economic benefits may reasonably be expected. By analogy the benefit and the expenditures by which it is measured can be regarded as the consumer benefit of avoiding the disease under study.

One difficulty with this process of valuation, it was noted, is that out-of-pocket expenditures are sometimes only a small fraction of a person's medical care expenditures. The remainder of the bill was paid earlier in the form of a general health insurance premium, and does not reflect the value placed on a particular service at the time of delivery. Out-of-pocket expenditures would constitute an understatement of the value, while the total bill might be an overstatement.

Nevertheless, this general formulation is valid: Suppose an individual were fully protected against earnings loss and all of his medical expenses were met by taxes; he would still be willing to spend some money to avoid illness, or disability, or premature death. Economists may differ on how to ascertain this sum of money, but they agree that it is greater than zero. Loss of output alone, or even in combination with medical care expenditures, understates the size of the economic loss attributable to a disease.

A serious research effort to measure the value of this intangible consumption benefit would be in order. It should be recognized, however, that precise answers cannot be expected.

As for interrelationships among diseases, there is no doubt that a person with two diseases has an increased risk of dying from either one of the diseases. For example, if a person has a bleeding ulcer and a weak heart, he cannot be operated upon, and the probability of his dying from the ulcer increases.

This is different from the matter of selection for survival. By the selection hypothesis we mean that people who formerly died from infectious diseases no longer do so and survive to die from diseases of the older ages. One implication of the hypothesis is that those who are saved are increasingly susceptible to disease in adult years. This is the argument advanced by Frangcon Roberts, the Welshman, who holds that all we are accomplishing is spending more and more money to care for sicker and sicker people. However, nobody really knows this to be so.

The economists asked the question: Why have advances in knowledge had so little effect on life expectancy at all ages other than infancy. One answer is that persons who survived to age 20 were always reasonably adapted to deal with their own germs. Another answer is that perhaps owing to changes in "living conditions," there has been an increase in lung cancer, in coronary heart disease, and in chronic bronchitis, which may have served to offset the declines in death rates from other causes. Still another answer is that applications of knowledge on a mass basis have yielded success at infancy. To deal with diseases in the older ages requires the application of knowledge on an individual basis, which is much more difficult to accomplish. In rheumatic fever, for example, the necessary knowledge exists, but it is not being applied. It is necessary to convince individuals to receive prophylaxis. If a pill were available to be thrown into the water supply, many more persons would receive prophylaxis.

Here the economists drew on an analogy. The profession usually takes the position that it is up to the economic system to provide jobs. A cost-benefit calculation for a disease control program assumes full employment. Similarly, it may be equally valid to argue that if medical research produces the knowledge that can reduce cancer by x percent, provided that smoking is curtailed, then the research has been successful. Failure of the public to apply the new knowledge is another matter, which may deserve separate consideration.

The economists agreed that with the data currently available, it is not possible to prepare economic guidelines for the allocation of funds to medical research.

It is recognized that the return on medical research is extremely difficult to measure. Accordingly, it is highly important that we base our decisions not on the greater feasibility of measuring certain effects, but rather on what the measurements would show if all effects were equally measurable.

QUESTION 5: What can be done to bring together the Federal Government's interests in medical research and in educating and training personnel?

One of the economists present advanced the proposition that investments in training for medical research and in research should be phased, with the former preceding the latter. In actuality, the Federal government has supported medical research, in preference to training.

An objection to this formulation is that it is not possible to train individuals for research apart from the performance of research. Are these not complementary activities at the graduate level of training? It is recognized, however, that formal academic training in subject matter and methodology can with advantage precede participation in a research project.

The economists pointed out that whether a physician cares for patients and disseminates the knowledge that exists or does research is an important economic decision for the country. The Federal Government affects this decision by providing funds for research.

The issue can be mitigated, however, by increasing the total supply of physicians. True, this takes place at the expense of something else, but perhaps this lies outside the health field.

One economist present reported that the United States stands alone among eight countries in having a declining ratio of physicians to population. One result is a reduction in the availability of medical services at night, weekends, and at home. Another is a rise in prices. It was noted, however, that, though declining, the U.S. physician to population ratio is still second or third highest among all countries. The fact remains, of course, that relatively little is known about the effect of some services on the health of a population.

Beyond the general issue of a limited supply of physicians for all purposes, there is another issue, that of the availability of personnel for research. Physicians who do research are not necessarily better, but they are specially qualified. Economists find it difficult to believe what is widely accepted in the health field, namely, that service, education, and research are so closely intertwined that a good physician can do all three with equal ease and competence.

The question was raised why a high-income profession like medicine should require the inducement of fellowships to attract recruits? One answer is, in order to match or overcome the attraction of physics and molecular biology. This may not be necessary, however. Biologists and physicists perform a good deal of medical research today. They have Ph.D. degrees, rather than M.D. degrees.

QUESTION 6: Can economists offer any guidance on the respective merits of project vs. program research financing?

The initial reaction was that there was no reason to make the choice all one way or the other.

One of the economists present set forth two propositions. One, in the form of a question: Is there ever a time when the Federal Government can say that the Nation needs a particular piece of research and then go out and solicit grant applications? If so, there is a clear case here for project support. Moreover, it would not seem to matter whether the method of payment is a research grant or a contract. Indeed, there is no reason why the contract mechanism cannot be combined with scientific review.

The other proposition is: If one always allows the investigator to apply for funds and the Government sets no priorities on problem areas to be investigated, then it would seem that a strong argument may be made in behalf of program support over project support.

The reason is that the method of financing research affects productivity. Under project financing there is instability of tenure; the writing of progress reports; the filing of applications some time before the current grant expires; and the tendency to set out to compile new data under a new grant, rather than conducting additional analyses of the data gathered under the old grant.

There was no consensus on these issues, in the absence of concrete knowledge of how the grants mechanism is operated and performs in practice.

Numerical Estimates of Economic Costs of a Disease

As part of the work of the President's Commission, the Health Economics Branch of the Division of Community Health Services of the U.S. Public Health Service was commissioned to measure the so-called direct and indirect costs of heart disease, cancer, and stroke. In view of the limitations of time, staff, and the state of the art, the Health Economics Branch defined its mandate as follows:

a. Calculate the medical care expenditures incurred by, or in behalf of, persons with heart disease, cancer, and stroke in the year 1962. Primary diagnosis (the final diagnosis for the condition that brought the person to medical attention) is the basis for classifying persons and their expenditures (or costs incurred in their behalf).

b. Calculate the loss of output by persons who died of, or were disabled by, these diseases in 1962.

Preliminary figures were prepared by September 30 and were distributed at the Conference. The results of the discussion may be summarized as follows:

Single year estimates of loss of output seriously underestimate the size of the economic problem.

Ideally, to help in policy formulation, the calculation should represent the present value of the future losses in output due to premature death in 1962. A less desirable alternative, but still a marked improvement over the single year estimate as a measure of the economic magnitude of a disease, is to calculate the loss of output in 1962 due to premature death in previous years by those who would have been alive and productive in 1962 if heart disease, cancer, or stroke had not intervened and caused them to die.

Looking toward the future, a social rate of discount should be applied. A discount rate is obviously not applicable to the losses attributable to past deaths, because all output is measured in the year 1962. The estimate oriented toward the future should allow for expected increases in the productivity of labor.

If it were known how many persons became permanently disabled in 1962, the same procedures would be applicable to morbidity as to mortality. In general, the most useful estimates for policy purposes would represent the present value of future losses in output attributable to the occurrence of initial episodes of disease in a specified interval. The basic data for preparing such estimates are usually lacking, however.

The same is true of medical care expenditures. Persons who suffered a heart attack in 1962 and survived may have expenditures in the future. These should be discounted to the present in order to calculate present value. Again the necessary data are lacking.

With one or two or three diseases taken at a time, one cannot be sure that the calculations of costs are performed consistently and correctly. To be assured of this it is necessary to perform a complete accounting for all categories of disease, however classified, at the same time. This appears to be a practicable procedure in measuring direct costs (medical care expenditures), where the validity of the procedures and the accuracy of the calculations can be confirmed by comparing the results with known totals for a single year. No such check is available when the present value of future earnings is calculated. For indirect costs, moreover, existing data are so much more fragmentary and unreliable as apparently to render the desirable step impracticable.

Questions were raised about the adequacy of the classification by primary diagnosis alone, but no decisions were reached or recommendations made.

Causes of death may not be truly independent. If so, the elimination of a given cause of death from the life table represents the maximum reduction in deaths that is attainable. The true gain is probably lower.

Several diseases frequently coexist in a patient. This means that to attribute the total costs to any single disease is to overstate the potential gain from overcoming it. Consistent adherence to classification by primary diagnosis has the advantage of counting a person only once. If diseases occur independently (which they do not), the overstatements and understatements will more or less balance.

It was recognized that the task undertaken by the Health Economics Branch was laborsome and complicated. Given the time limitations, it was well performed. Some of the desiderata stated above could only be incorporated in another future project.

Problems for Research

It is useful to isolate and present some of the problems that were proposed for future research.

1. What has been the return on investment in specific research projects? How often has the return accrued in the categorical fields that sponsored the research?
2. In calculating the present value of future losses and gains, take into account the losses due to future mortality, future morbidity, and future medical care expenditures of cases that newly occurred in 1 year, say 1962.
3. A complete accounting of direct costs by disease category would be useful and is practical. A similar accounting of indirect cost would be interesting, is perhaps not so practical, and may not be equally meaningful.
4. What are some practical approaches to calculating the economic value of relief from pain, grief, etc? Empirical work is indicated.
5. One way to approach the issue of general vs. categorical support for research is to review and compare the quality of rejected applications for research support in various fields.
6. Economists need data on the effect of services on the health of a population, which they are not equipped to compile.

APPENDIX

Questions for Discussion

1. Is it meaningful to ask of economists, how much can this Nation afford to spend on medical research? If this is not a good formulation of the question, what would be a sensible way tc ask, how much should we spend on medical research 10 or 15 years hence?

Background Issues:

a. What are the dimensions of the social welfare function, or what is our conception of it, as it relates to medical research expenditures?

b. If the Nation can afford more expenditures on medical research, why aren't these added expenditures being made? Lack of information? Why should these added expenditures be made?

c. In assessing the amounts we should spend on medical research, how do we evaluate the costs and returns of this activity against other types of research and development and against all other types of expenditures.

- Nature of the production function for medical research.
- Measurement of "'physical output" of medical research.
- Possibilities for successful exploitation of research findings.
- Evaluation of economic benefits (reduction in economic losses arising from the incidence of a disease) of medical research and associated health expenditures.
- Measurement of benefits, both direct and indirect.
- Timing of benefits.
- Choice of discount rate.

d. How can current evidence be used as a basis for allocating expenditures in the future—over the next 10 or 15 years?

2. Currently government (mostly Federal) pays for two-thirds of all medical research expenditures. Is it reasonable to project this figure into the future? In other words, are there any economic criteria for determining what government should pay for in medical research and what business and philanthropy might be expected to do? Moreover, within the public sector, is there a proper role for State and local government.

Background Issues:

a. In what sense is the present distribution of expenditures optimal?

b. What is the expected division of financial support between government and other sectors?

- In applied research we might expect a more-nearly "adequate" amount of support from the private sector but considerable underinvestment in basic research. For foundations the reverse is likely to be true but their financial resources are quite limited.

c. Thus, what is the rationale for government financial support of medical research?

- Uncertainty elements.
- Appropriability of benefits.
- "Public goods" aspect.
- External effects.
- Economies of scale.

d. Might there be reasons for or methods of stimulating the financing of more research by the private sector?

e. Is the role of State and local governments likely to be limited except where the regional incidence of a disease or affliction is high, e.g., air pollution in Los Angeles?

3. Are there criteria to help determine how much of government's medical research expenditures should be devoted to broad general research and how much should be devoted to special areas, such as heart disease, cancer and stroke?

Background Issues:

a. Has basic medical knowledge advanced to the point where funds should be concentrated in an attack on specific diseases?

b. To what extent are basic research expenditures complementary to specific disease research expenditures?

c. What are the prospective costs of and benefits from an expansion of specific research with a simultaneous contraction of basic research, and vice versa?

d. How are intellectual resources reallocated in response to the changing "mix" of basic versus specific medical research support?

4. Given the fact that some research will be conducted in special areas, is it possible to determine on a rational basis how much to spend in each area by comparing the respective sizes of the problems? To the extent that this may be so, how much weight should one assign to the economic effects of disease and how much to other factors, such as uncertainty of discovery, pain on the part of patients, sense of bereavement on the part of family, etc. * * *? Or would one say that some of these concerns lie outside the economist's purview?

Background Issues:

a. Are there greater gains to be had from seeking cures in one disease area as compared to another?

b. What is the most appropriate measure of the economic losses associated with a disease, e.g., number of deaths, number of work-years lost, economic value of work-years lost, etc.?

c. How should the probability of success and the costs of a special disease area program be stacked up against its benefits, i.e., the reduction of economic losses associated with the disease?

d. How do we take account of the interrelationships among diseases? For example, eliminating one disease reduces an individual's susceptibility to other diseases.

e. How much are we prepared to spend in attempting to eliminate first one specific disease and then another, particularly those affecting the aged, given the inevitability of death?

f. Can any monetary equivalents be assigned to the intangibles associated with the incidence of disease?

5. In order that research continue, it is essential to reproduce investigators. Can you suggest a rational framework for linking the Federal Government's efforts in research and in training professional and technical personnel?

Background Issues

a. Should some funds be allocated to research on methods of organizing medical practice more efficiently, so as to make additional personnel available for research work? Will the development of emergency branch hospitals, clinics, etc., help accomplish this?

b. Will greater efforts be made to train additional investigators if grants carry the proviso that a training program must represent a part of the grant activity?

c. As medical research expenditures rise and training grants increase, how do we insure that more people are trained than was the case previously? Or are research fellows merely better paid than before? To the extent that more people are being trained, has it been at the expense of some other related field?

d. Will the establishment of more institutes and centers further training efforts?

6. The method of financing research affects recruitment, retention, and productivity of research personnel. Can you offer some guidance regarding the conditions under which project research financing is preferable to program support, and vice versa?

Background Issues:

a. What are the advantages and disadvantages of program versus project support? In:

- Continuity.
- Flexibility.
- Control.
- Costs of administration.

b. What is the desirability of program versus project support? For:

- General research.
- Specific area research.
- Training of research personnel.
- Research performed by (a) industry, (b) government, (c) universities, and (d) nonprofit institutions.

DR. HERBERT KLARMAN.

Research and Economic Growth—What Should We Expect?

B. R. Williams

The use of science in agriculture, industry and medicine has made possible enormous increases in population, material standards of living, health and the expectation of life. We can expect further increases—provided that we use science for economic growth and not for nuclear destruction.

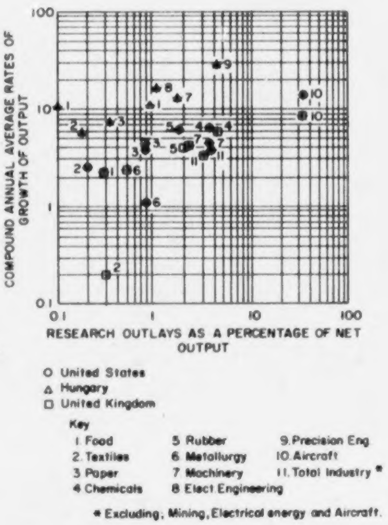
It is widely believed that the key factor in this growth is the rate of expenditure on research and development. It is also widely believed that the proportion of national output devoted to research and development is critical. Think how often it is argued in Britain that growth is held down by a failure to spend on research and development as high a percentage of national product as do the Americans. In France (and Germany and Australia) the argument tends to be that growth is held down by a failure to spend as high a percentage as the British. Recently, a leading member of the French planning commission assured me that finding the appropriate level of expenditure on research in France was really very easy: "for where expenditure is below the corresponding British level you know that it should be increased."

There is, however, no obvious logical step from the observed effects of applied science on past growth to the conclusion that *national* expenditure on *research* and *development* is the key to future national growth. Research is simply a process of adding to scientific knowledge. Sometimes new scientific knowledge has a direct influence on the technology embodied in production processes. Frequently, however, a further (and often very expensive) activity called development is required before science can affect technology. Hence the need to distinguish between science—the sum total of systematic and formulated knowledge about the real world—and technology—the sum total of formulated knowledge of the industrial arts. It is technology, and the efficient use of it, that is critical in growth. What evidence is there that in this sense the use of science is a certain derivative of current or recent research and development expenditure?

Evidence of Growth as a Function of Research and Development

In a paper on "The Economics of Research and Development"[1] Dr. J. R. Minasion set out to test the hypothesis "that productivity increases are associated with investment in the improvement of technology, and the greater the expenditures for research and development the greater the rate of growth of productivity." Minasion used a cross-section study of 18 firms in the chemical industry for the years 1947–57. He found that "the relevant research and development expenditure was a highly significant independent variable explaining not only the rate of growth in productivity but also the trend of the profitability of 18 chemical firms in the sample." He also found that lagged research and development explained end-of-period profitability better than end-of-period research, and development was explained by lagged profitability and that there was not a statistically significant relationship between the growth in productivity and the rate of change in output, average plant size, or the rate of growth in plant size. However, Minasion's sample was not necessarily representative of the chemical industry and he was unable to isolate factors such as royalty payments, which would almost certainly have influenced productivity changes. Nor, of course, could these results be legitimately generalised for the whole economy.

DIAGRAM I



If we shift the measure from growth of productivity to growth of output, we do, however, find some broad but striking relationships between growth in output and research outlays as a percentage of net output. In Diagram I this relationship is plotted for 10 industry groups in the U.K., U.S.A, and Hungary for the years 1949–59.

Dr. R. A. Ewell had earlier pointed to this relationship between research and growth in U.S. industry from 1928–53.[3] He also pointed to a high correlation between the growth of national product and the country's expenditure on research and development. Assuming a causal link between research expenditure and growth he forecast that, if (on 1954 prices) research and development expenditures were increased to $6.3–6.9 thousand million by 1964, gross national product (G.N.P.) would rise to $500 thousand million. Roughly the Ewell projections were:

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[1] Minasion, J. R., "The Economics of Research and Development," in *The Rate and Direction of Inventive Activity*, Special Conference Series No. 13 (Princeton: National Bureau of Economic Research, 1962), pp. 93–142.

[2] From U.N. report on *Some Factors in Economic Growth in Europe During the 1950s;* (Geneva: United Nations, 1964), Chapter 5, p. 10.

[3] Ewell, R. A., "First outpost in a new frontier," *Chemical & Engineering News*, 18 July, 1955.

TABLE I

	1954	1960	1964
G.N.P. index	100	114	131
R. & D. as a percentage of G.N.P.*	1.1	1.23	1.37

Ewell's confidence in these conclusions was increased by an alternative line of calculation—that between 1928 and 1953 new products created by research and development were between 11 and 22 percent of G.N.P. and that without them annual growth would not have been 3 per cent but 2½–2 per cent. By imputing this growth to the median research and development expenditure for the period, he concluded that the annual yield to expenditure on research and development was 100–200 per cent. In this calculation of yield Ewell made no allowance for outlays other than those on research and development, although on his calculations $11 of capital expenditure were required for $1 of research and development.

Implications of This and Other Evidence

Minasion's conclusions were based on a very limited sample—18 U.S. chemical firms for the years 1947–57. In Britain for the period 1949-59 no such conclusions emerged from the Freeman and Evely analysis of a sample of 44 firms in general engineering, 22 in chemicals, 12 in electrical engineering and 17 in steel. For the sample firms in chemicals and general engineering there was a positive association between growth, profitability and research ratios for the top 5 per cent and bottom 5 per cent of firms, but not throughout the whole range of firms. For the whole sample "additional research and development appears to make only a limited contribution to additional growth. The greater part of the differences between firms in rates of growth and profitability are due to other factors than differences in the amount of research and development done."[4]

The information in Diagram I is also rather limited. Correlations in this field are sensitive to definitions of industries. Industries as defined for statistical purposes vary greatly in their degrees of integration and it is possible for an industry with a high rate of growth in output and productivity to depend on the innovatory activities of its suppliers. Thus it matters greatly whether or not one defines the motorcar industry in Britain to include the firms which make the components.

However, the main problem here is one of interpretation. Should we conclude, or imply, that if the low-growth industries had spent more on research and development they would have grown faster? The issue is not simply whether more physical output could have been produced but whether more output could have been profitably produced.

If economic growth opportunities were there but wasted by the industries with low percentage expenditures on research and development, this could mean that firms in such industries had failed to recognise (and/or exploit) opportunities for profitable investment in research and development. Or, it could mean that although the potential was there it was not possible for individual firms in the industry to realise it. This could happen where the firms in the industry were too small to finance and manage the research and development required, in which case state provision for research and development (as in agriculture), or cooperative research (as in steel, textiles, pottery, etc.), should solve the problem.

There is little doubt that many firms in low-growth industries have often failed to recognise opportunities to invest in research. But whether British textile and metallurgical firms have been worse in this respect than paper firms (see Diagram I), I very much doubt. In any case one of the striking things about research and development percentages in different countries is that their industrial ranking is not very sensitive to the size of firms in the industries. This is implied in the following table which compares U.S. and British firms.

TABLE II

Research and Development as Percentage of Net Output, 1958[5]

	U.S. Companies	U.K. Companies
Aircraft	30.9	35.1
Electronics	22.4	12.3
Other electrical	16.3	5.6
Vehicles	10.2	1.4
Instruments	9.9	6.0
Chemicals	6.9	4.5
Machinery	6.3	2.3
Rubber	2.7	2.1
Non-ferrous metals	2.0	2.3
Metal products	1.3	0.8
Stone, clay and glass	1.2	0.6
Paper	0.9	0.8
Ferrous metals	0.8	0.5
Food	0.5	0.3
Lumber and furniture	0.2	0.1
Textiles and apparel	0.2	0.3
All industries	5.7	3.1

The similarity in the pattern of industrial research and development between the U.S. and Britain is very striking. The only significant difference is in vehicles, which is partly explained by differences in industrial coverage. The main reason for the differences between industries is that the average profitability of research varies from industry to industry, depending on the state of technology and the extent of market saturation. It would clearly be quite inappropriate to conclude from the evidence of Diagram I that growth rates can be pushed up simply by raising research and development percentages.

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[4] *Industrial Research in Manufacturing Industry 1959–60* (London: Federation of British Industries, 1961), pp. 43–49.

[5] From Freeman, C., "Research and Development: A Comparison between British and American Industry," *National Institute Economic Review* (May, 1962), 20, p. 31.

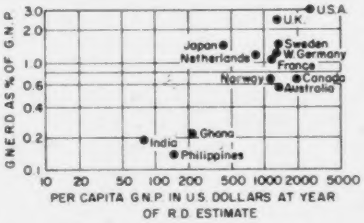
An appraisal of Ewell's "evidence" helps to throw further light on this issue. Ewell's forecasts proved to be very wide of the mark. His forecast was that in real terms research and development would rise by 27 per cent and G.N.P. by 14 per cent. In fact, research and development rose by over 150 per cent and became 2.8 per cent of G.N.P., which rose by only 5 per cent.[6] In 1964 research and development will be more than 3 per cent of G.N.P., although G.N.P. will be little above Ewell's prediction for 1960. Now it may be argued that this unexpected rise in research and development expenditures after 1954 could not yet have had its full effect on growth. This may prove to be so, but the fact is that growth has not risen to the levels expected from past research expenditure. In the period up to 1954, almost one half of America's cumulative expenditure on research and development had been in the last five years. Allowing a 5–10 year time lag, the growth effects Ewell confidently forecast from research should have shown at least from 1960 on.

The basic error in Ewell's approach was that he used a bi-variate approach to a multi-variate situation. He mentioned the importance of capital expenditure, production, sales, advertising, management, etc., but implied that we can simply take them for granted. He even calculated the yields to research and development as if they were the only costs of new innovation. Ewell did not discuss the objectives of research and development, whether certain types of research are more likely to have growth potential than others, the best mixtures of research and development, the alternative uses of scientists and engineers, or the possible tendencies to diminishing returns. By now it is clear that these are important factors, and that the model implied by Ewell—which is still frequently used—is far too simple to cope with the complex relations between science and growth.

We get a further indication of the complex relations between research and growth from the relations between G.N.P. per head and the percentage expenditure on research and development in different countries. These, which are shown in Diagram II, are sometimes taken to demonstrate the tendency of the R. and D. percentage to rise with G.N.P. per head.

A tendency can be weak or strong. The tendency shown in Diagram II is very weak. Japan is obviously well "off trend." So too are Canada and Australia. Indeed, if a vertical line is run through the West German per capita G.N.P. it is plain that for six countries with similar levels of G.N.P. per head, R. and D. percentages range from 2.5 to 0.6. If, furthermore, at 1961 prices the U.S. per capita G.N.P. back to 1954 and the corresponding R. and D. percentages, were superimposed on Diagram II, we would see the R. and D. percentage move from 3 to 1 per cent with scarcely visible change in per capita G.N.P.

DIAGRAM II



After this discussion it should come as no surprise that internationally there is no sign of a high correlation between rates of growth in output per head and the percentage of G.N.P. devoted to research and development. The effect of plotting the research and development percentage against the annual percentage growth of national product per man for nine countries is shown in Diagram III.

It may be argued that plotting research and development rates against growth rates for the same years is rather meaningless. This is a valid criticism (which should also be applied to Diagram I) if relative research and development rates have changed significantly. Research and development statistics in most countries are not very accurate but such evidence as there is suggests that lagging scrambled research and development figures five or 10 years behind the growth figures would not materially alter the picture.

In any case, we have very little clear-cut evidence about the appropriate time lags, which are not the same for research and development and probably not the same in different countries and over time. It is true that some research will have a direct impact on technology. Thus, successful research into optimum firing conditions in pottery could be applied directly to operating conditions in tunnel ovens. But for basic and background research and most industrial research, the impact on technology, if any, is routed through the development process. This may be a very expensive business—as, for example, the building of prototype nuclear reactors of different kinds at Calder Hall, Dounreay and Winfreth Heath. It may also be a lengthy business. Although nuclear reactors have been built for the Central Electricity Generating Board to operate commercially they are not in fact competitive with traditional forms of power. It is probably wise to regard the nuclear stations as part of the development process required to get that experience of building and operation which may make nuclear power costs just break even with, and then undercut, conventional forms of generation.

Recent expenditure on development is more likely to affect contemporary growth than equally recent expenditure on research. In considering the impact of research and development on growth this might not matter if basic research, applied research and development occurred in fixed proportions. But they do not. The actual proportion appears to vary over time and between countries. Due to difficulties of classification, estimates of the research and development proportions in different countries are subject to wide margins

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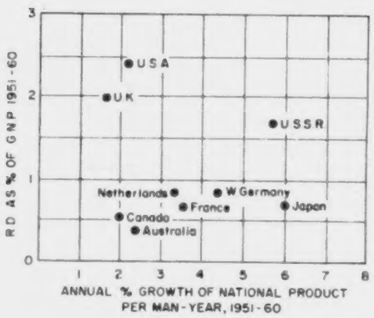
[6] The degree of error is so large that the revision of 1954 R. and D. expenditure, bringing it to 1.4 percent of G.N.P., does not make a great deal of difference.

[7] From *Science, Economic Growth and Government Policy* (Paris: OECD, 1963).

DIAGRAM III



of error and the figures given below should, therefore, be treated with some caution. Nevertheless, the possible errors of estimation are not large enough to destroy the general picture.

Research and development estimates are also scrambled in another important sense. The objectives of research and development also vary. It has been estimated that in the U.K. only 50 per cent of research and development has a growth objective.[10] Insofar as defence research and development (which until recently was over one-half the total expenditure in the U.S. and the U.K. and is now over one-third) contributes to growth it is in the nature of "fall out" or "spin off," which has not apparently been large. It is not then at all surprising that scrambled totals of expenditure on research and development have not shown a significant correlation with growth in national productivities.

Alternative Uses of Scientists and Technologists

It is now time to consider the implications of choice in the deployment of scientific manpower. Scientists and technologists are not only used in research and development. They are also used for the actual introduction and operation of new technologies. For example, in the chemical industry only about 50 per cent of the scientists and 20 per cent of the technologists employed are engaged in research and development. By contrast, in scientific instruments and electronic instruments the corresponding percentages are approximately 75 and 50. Given this sort of variation it is clear that the appropriate distribution of scientific manpower varies with industrial structure. But unless the distribution is appropriate and it would be very rash to assume that in each country it always is, growth potential will not be realised. From the need for scientists and engineers to introduce and operate sophisticated technologies there arises the possibility that research and development may be excessive. It will be excessive, that is to say will actually hinder growth, when the output of potentially usable new technology is greater than the capacity of industry to absorb and when the use of more scientists and technologists outside research and development departments would increase industry's capacity to carry through technological change.

The possibility of excessive expenditure on research and development is increased by the fact that research and development do not have to be home grown. Unless a country is leading in all fields of science and technology, there must always be some choice between making at home and buying abroad. Now the less advanced a country's technology and the smaller its supply of scientific manpower, the greater the advantage of importing science and technology and of using a higher proportion of scientific manpower outside research and development activities.

Consider the Australian case. Because the Australian population is very small there is no chance of an effective research and development effort in more than a small part of the field. Furthermore, the stock of scientists and engineers per head of working population in Australia is only half that in Britain. It follows that the real cost of using scientists and engineers in industrial research and development and particularly in research is, in general, much higher in Australia than in England. In fact Australia achieves a higher growth rate with a very much lower research and development percentage than Britain and the United States. Part of the explanation is that the Australian industrial pattern is different. If, for example, each industry (including agriculture) devoted to research and development the British proportion of net output, Australian expenditure as a percentage of G.N.P. would be only (defence complications apart) 60 per cent of the British level. But this is not the main part of the explanation. The science-based industries in Australia—vehicles, chemicals, electrical—are dominated by overseas companies and this, by reducing the need for home-grown research and development, increases the effective supply of scientific manpower. If in those industries the expenditure on research and development were at the British percentage of net output, research and development expenditure in Australian industry would be about 200 per cent greater than it is. As it is, the subsidi-

TABLE III

Estimated Percentage Distribution of R. and D. Expenditure in Four Countries[9]

<table>
<tr><th></th><th></th><th>Basic</th><th>Applied</th><th>Development</th></tr>
<tr><td>United Kingdom</td><td>(1961)</td><td>11</td><td>25</td><td>64</td></tr>
<tr><td>United States</td><td>(1959)</td><td>8</td><td>22</td><td>70</td></tr>
<tr><td>France</td><td>(1959)</td><td>26</td><td colspan="2">74</td></tr>
<tr><td>Australia</td><td>(1961–62)</td><td>25</td><td>35</td><td>40</td></tr>
</table>

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[8] From Williams, B. R., *Investment and Technology in Growth* (Manchester: Statistical Society, 1964).

[9] From *Annual Report of the Advisory Council on Scientific Policy 1961–1962*, Cmnd. 1920 (London: H.M. Stationery Office, 1963); Keezer, D. M., "The Outlook for Expenditures on Research and Development during the Next Decade," *American Economic Review*, L (May, 1963); *L'Usine nouvelle*, 18 May, 1961, p. 29; Williams, B. R., *Industrial Research and Economic Growth in Australia* (Adelaide: The Griffin Press, 1962).

[10] Carter, C. F., and Williams, B. R., *Government Scientific Policy and the Growth of the British Economy* (Manchester School of Economic and Social Studies, 1964), reprinted from *Manchester School of Economic and Social Studies* (September, 1964).

ary companies are able to economise research and development by adopting or adapting the technologies of the parent companies.[11]

Of course, there are other ways of acquiring new technologies from abroad. Licences to produce patented processes and products and the purchase of "know-how," play important roles in the diffusion of technology. Sometimes licence terms, even the grants of licences, depend on the ability to offer scientific or technological knowledge in exchange. But this is not the usual procedure. Thus, in 1961, the United States received from the recorded sale of licences and "know-how" $577 million, but paid out only $63 million. In 1963 Western Germany received DM200 million for patents, inventions and processes, but paid out DM550 million. Germany's payments for proven new technologies amounted to approximately one-sixth of its total expenditure on research and development, some considerable part of which will have no impact on growth. Clearly then the impact of foreign technology on German growth is even greater than the actual payment of DM550 million would at first sight suggest.

The following crude calculation helps to give some idea of the significance of this West German expenditure on foreign technological knowledge. British research and development is over 2½ per cent of G.N.P., the German just over 1 per cent. Now if we deduct and purely defence element in this and add in expenditure on foreign technological knowledge (multiplied by two in recognition of its proven impact on technology), the combined figure is approximately the same percentage of G.N.P. in both countries. If, as could well be argued is appropriate, a higher multiplier was used for expenditure on foreign "know-how," the German percentage would be higher. But through these differing combinations of home-grown and imported technologies in the two countries the deployment of scientific and technological manpower differs greatly. The stock of scientists and engineers as a percentage of the labour force appears to be no lower in West Germany than in Britain.[12] It follows that Germany uses a considerably higher percentage of its qualified scientists and engineers outside research and development than does Britain. There is nothing in West Germany's record of postwar growth to suggest that this policy (which many scientists regard as parasitic) has not paid handsomely. Equally, there is very little in Britain's record of postwar growth to suggest that her policy has paid off.[13]

The Significance of R. and D. Percentages

There is no inherent virtue in high R. and D. percentages, just as there is no inherent virtue in high rates of capital expenditure. They are both parts of the investment process and as such constitute costs of growth. Optimum allocation of resources involves taking investment in R. and D., as well as in plant and equipment, to the point where prospective yields to additional expenditure equal the cost of the finance involved. The existence of average yields to research and development higher than the cost of additional finance does not as such indicate under-investment. The crucial thing is the marginal yield, which may quickly fall below the average yield because of the limited rate at which production and marketing departments can absorb new knowledge, install new processes and market the additional (or different) outputs. This rate is itself affected by the distribution of scientific and technological manpower within the firm (and even on occasions within firms expected to buy the new products). Given the low elasticity of the supply of scientists, the time required to absorb newcomers, and possibly the need to recruit less able scientists and technologists, marginal yields to research in fields where both the market and technological potential for innovation is high will usually be very much lower than the average yields.

What can be said about the R. and D. percentage at which marginal yields will become equal to the cost of finance? Is there some basic tendency for the R. and D. percentage to grow with per capita G.N.P.?

We have seen that there are very wide differences in R. and D. percentages in countries with similar levels of per capita income and that national R. and D. percentages are not closely correlated with growth. Nevertheless, in almost every country with a growing standard of living, the ratio of R. and D. expenditure to G.N.P. has shown a strong upward tendency.[14] What is this due to and how far can we expect the rise to go?

Leaving aside military R. and D., the growth of which in the last five years has fallen behind civil R. and D., we can impute the rise in the R. and D. percentage to three factors. Because manufacturing industry generally has a much higher ratio of research to output than primary and tertiary industries, industrialisation, which brings an increase in the relative importance of the manufacturing sector, usually pushes up the R. and D. percentage. This, however, is a transitional influence and does not apply to countries which are highly industrialised. In such countries, a rise in the R. and D. percentage is due both to a shift within the manufacturing sector and to the "discovery" of research in the traditional industries.

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[11] See Williams, B. R., *Industrial Research and Economic Growth in Australia* (Adelaide: The Griffin Press, 1962).

[12] See *Resources of Scientific and Technical Personnel in the OECD Area* (Paris: OECD, 1963).

[13] The failure of our massive expenditure on research and development to produce a dramatic change in the economic position has produced a certain gloom in "the scientific world." But cheerfulness about the future keeps breaking in. For example: ". . . while spending on all R. and D. doubled between 1956 and 1962, spending on civil industrial R. and D. actually trebled. In this connection I would like to refer to a leader in *The Times* of 19 September, entitled 'Brighter News,' which discussed the recent encouraging rise in British exports and output. The writer sought an explanation of Britain's improving competitive position in the restraint of costs at home and in the increasing wage pressures on the Continent. While these may well be important and essential components, I would myself be interested to know how much our improved position can be attributed to this rapid increase in expenditure on industrial research and development to which I have just referred. Certainly we would expect that this investment would now be beginning to pay dividends." Sir Harry Melville, "Industrial Research and Development in Britain" in *Science and the City* (London: Harrison, Raison & Co., 1963), p. 41. Since Sir Harry wrote this, the performance of industrial production and exports has been very discouraging.

[14] See *Science, Economic Growth and Government Policy* (Paris: OECD, 1963), pp. 22–23.

A shift within the manufacturing sector (implied in Diagram I) towards the research-intensive industries is the effect of three factors. The first is the effect of industrialisation in poorer countries on the industrial structure of the richer countries. This industrialisation undermines the international trade position of the richer countries and forces them to specialise more in those industries which require higher rates of investment in plant and in people.[15] On the whole these are the research-intensive industries—though, as we have seen, part of the required research and development can be imported. The second factor is the tendency for people in rich countries not to react to their riches by demanding more leisure rather than more goods. This makes it profitable to invest in inventing new commodities. The third factor is the effect of higher incomes on investment in people, including the training of scientists and engineers. Here, as in the case of R. and D., the relation is very general. Thus, the Russian stock of scientists and engineers as a percentage of working population is higher than the British despite a considerably lower level of income per head. This sort of variation is indeed an important factor contributing to the confused position pictured in Diagram III. But given this greater supply of scientists and engineers, the capacity of countries to invent potential new products and processes and to use them efficiently is increased, so raising marginal yields to investment in technology.

There is, however, no reason to expect this process to continue unchecked. For, although some of the new processes and products created by research and development do not require capital expenditure to bring them into effect, many, perhaps most, of them do. Even if, as there is no reason to believe, the supply of very creative scientists and engineers increased proportionately to the overall supply, limits to the rate of growth in capital expenditure would set limits to the growth of research and development. In the United States, Professor Yale Brozen has argued that the correlation between research expenditures and subsequent sales has tended to weaken since 1957, and this is a sign of research and development expenditure moving nearer to an equilibrium situation. In 1960 he predicted that the portion of national product devoted to research and development would rise from 1.9 to 3.0 by 1975 with a ceiling of 5 per cent in the next century.[16] His 3 per cent has arrived a decade early, partly because, like Ewell, he overestimated the growth of national product. This is an important point, for nothing pushes up research and development percentages faster than a failure to achieve the growth expected from research.

What Should We Expect from Research

Countries differ in levels of income, in levels of technology, in industrial structures, in size, in the proportion of scientists and engineers in the work force and so on. These differences affect the R. and D. percentages at which the yields on additional research fall to a low level; the ratios of home-grown to imported science and technology; the appropriate distribution of expenditure between basic and applied research and development and of manpower between research, development and other activities.

In a country such as the United States, with a relatively high proportion of scientists and engineers in the work force and a technological lead in most fields, a high R. and D. percentage will be consistent with an attempt to achieve high growth and a condition of it. A high R. and D. percentage will not leave a critical shortage of men required to install and manage new technologies. And given the restricted opportunity to borrow new technologies from abroad, a big development effort will be required to to create the new technologies. As the country gets richer there is likely to be an increasing struggle to maintain a high growth rate as trade positions in existing export products are weakened by the diffusion of its new technologies abroad and higher investment rates are required to establish the still newer technologies. Just how all this will affect the total relations between R. and D. percentages and growth will depend on a very large number of factors, including trade policy, population growth, military expenditures and future income elasticities of demand for leisure. If, for example, the demand for leisure increased, we could expect relatively greater emphasis on labour-saving process innovations, which would have a bigger effect on product per man hour than on product. In other words, product per man hour could grow rapidly, even though G.N.P. did not.

Between the countries of Western Europe and the United States there are important differences, which should affect what we expect from research. The fields where any country has a technological lead over the U.S. are few and in all countries the stock of qualified manpower is a smaller percentage of the work force than it is in the U.S., and very much smaller anyhow. It follows that no country can afford to try to cover the whole field of industrial research and development;[17] that any such attempt must lead to a weak effort and, therefore, a low R. and D. yield in all fields. It follows too from the possibility of importing technology from the U.S.—whether in the form of payment for technical knowledge or the willingness to allow the operation of American subsidiaries—that the dependence of innovation on home research and development is less than in the U.S.[18] Just how far it will pay a West European country to import technology will depend on the cost of acquiring it and the efficiency of home R. and D.[19] It is thought, for example, that the recent rapid rise in Japanese research and development has been connected with a rather steep rise in the cost of buying "know-how."

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[15] See *Industrialization and Foreign Trade* (Geneva: League of Nations, 1945).

[16] Brozen, Yale, "The Future of Industrial Research and Development" in *The Rate and Direction of Inventive Activity*, pp. 273-276.

[17] Indeed, in some cases even one field is too expensive. Hence the collaboration between France and Britain in developing the Concord and between the Common Market countries in the development of nuclear power.

What follows about the distribution of qualified manpower between research and development and other activities is not so clear. For, as we saw in the case of Australia, the use of foreign subsidiaries in research-intensive industries has the effect of adding to the effective manpower supply. But the probability is that the most economic use requires a smaller proportion of scientists and engineers in research and development than in the United States.[20] On the other hand, purchasing "know-how" and giving foreign firms with advanced technologies facilities to operate, may have a greater economising effect on development than on applied research. The precise effect will, of course, depend on the fields in which most use is made of foreign development work. But certainly the effect could be to produce more research in relation to development than would be sensible in a closed economy.

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[18] This is one reason why correlations between R. and D. and profits are higher in the U.S. than in Britain.

[19] For an account of the great impact of U.S. firms on British innovation see Dunning, J. H., *American Investment and British Manufacturing Industry* (London: George Allen and Unwin, 1958).

[20] Some of the implications of all this for government scientific policy in Britain have been examined in Carter, C. F., and Williams, B. R., *op. cit.*

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United States Science Policy: Its Health and Future Direction

Donald F. Hornig

It is timely that this review of U.S. science policy is being held in December 1968 just before a change in the U.S. Presidency—a time when special thought and attention are being given within and outside of government to the health and future directions of U.S. science policy. For myself, I look forward to contemplating what "they" should do rather than trying to get things done myself in a very complex government.

In a sense, such review and evaluation is a continuous process, but I am struck by the fact that there have been discontinuities in this process at roughly 5- to 6-year intervals since 1940.

The first major appraisal of U.S. science came immediately after World War II. Under the Office of Scientific Research and Development we had built from scratch a magnificent team of scientists and engineers and an array of first-class laboratories. In 1945 we saw the scientific team being disbanded and the research facilities transferred to other auspices. These circumstances were the cause of much thought and debate, which produced such appraisals of the needs and deficiencies of American science as Vannevar Bush's *Science: The Endless Frontier* (*1*), which still makes good reading, and the well-known Steelman Report for President Truman.

In 1951, under the stress of the Korean War and the possibility of another mobilization of the scientific community, President Truman created a Science Advisory Committee in the Office of Defense Mobilization, to provide the President with independent advice on scientific matters, particularly those of defense significance. This was the first significant step toward moving scientific advisers into the White House.

Again in 1957, in the traumatic aftermath of Sputnik, there was a call for a general reappraisal of where we stood in our national science policies and goals and the adequacy of government science organization. It resulted in the appointment of the first full-time Special Assistant to the President for Science and Technology, James Killian. Simultaneously, the President's Science Advisory Committee was established in the White House.

Five years later, in 1962, after another study and review of the White House science organization, it was decided to establish the Office of Science and Technology (OST) to provide permanent staff resources to the President for dealing with matters involving scientific and technological considerations.

Now, 6 years after the OST was created, we are again at the crossroads of introspection and examination of our national science policy and the organization needed to formulate it and carry it out. It is my feeling that, as before, changes will be made—and, I hope, for the better.

Having spent the past 5 years at the bench of U.S. science policy development, I would like to review with you some of the issues and problems as I see them, with some thoughts as to the future.

The main problem areas have been perceptively identified by the OECD (Organization for Economic Cooperation and Development) examination of U.S. science policy: academic science and the universities, the role of the government in industrial research, some of the social impacts of U.S. science policy, and the adequacy of the mechanisms in the U.S. government for dealing with these problem areas—that is, to make science do for the intellectual and material welfare of the American people all the things we think it can do and that we claim for it.

Federal Support of Academic Science

With regard to academic science and the universities, the central questions are: first, how to provide training of high quality for enough scientists and engineers of the right kinds; second, how to maintain vigor and creativity in the basic research establishment; and third, how to set priorities and determine the relative emphasis given to different research areas.

Concern over maintaining the vigor and quality of academic science is not a new phenomenon in 1968. At each of the 5- to 6-year steps in the evolution of the government science structure to which I referred there was a peaking of public concern about the state of American science. I venture to say that this recurrent, if not continuing, concern will remain with us for the foreseeable future.

You will recall the pronouncements after World War II about the sad state of fundamental research in the United States and our unhealthy dependence on European scientific discoveries for the development of the U.S. arsenal of

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This article is adapted from an address presented 29 December 1968 at the Dallas meeting of the AAAS. At that time the author was President Johnson's Special Assistant for Science and Technology. He is now vice president of Eastman Kodak Company, Rochester, New York, and a professor of chemistry at the University of Rochester new weapons, most notably the atomic bomb. The case for substantially strengthening the ties of government to university science was eloquently stated in Bush's *Science: The Endless Frontier*, in July 1945.

The year 1950 finally saw the creation of the National Science Foundation, after long debate (and a Presidential veto) over how independent this so-called independent government agency should be.

Again in 1957, wtih the advent of Sputnik, there was a resurgence of concern and interest in academic research, particularly in terms of the production of new scientists and engineers with advanced training, partly out of fear that the rapidly increasing output of scientists and engineers in the U.S.S.R. would pose a long-term threat to U.S. security.

When I entered the White House scene I was confronted with the issue of academic science in a somewhat different form. The explosive growth of government support of science in the 1950's and early 1960's had left in its wake a new array of problems of science administration, both in the universities and in the government. There was evidence of congressional dissatisfaction with what they believed to be lack of tightness and tidiness of federal controls over these large expenditures. This was, in part, based on misunderstanding of the nature and form of federal support. The question of overhead rates charged by the universities was raised, apparently from a confusion of overhead and profits—a question, I must admit, that has not been swept away (witness the recent Mansfield amendment to limit indirect costs paid under research grants).

Members of Congress had become acutely conscious that university science had entered the big league of congressional interests. The House established a Select Committee to Investigate Expenditures for Research Programs. The House Science and Astronautics Committee moved to establish a permanent Subcommittee on Science, Research, and Development. The Congress debated ways of strengthening congressional mechanisms for obtaining information and advice on scientific and technological fields.

Today there are again mutterings about a "crisis of confidence" in federal support of academic research. As in the past, this appears to be another moment of introspection, calling for self-renewal and readjustment of our sights to see clearly the goals ahead.

The current problems of academic science *appear* to have their origins in the budget stringencies growing out of the Vietnam war. But, in my view, the budget squeeze is only one symptom of a more general difficulty. It has brought to the surface the latent, unresolved problems which must inevitably be dealt with directly. I refer to such issues as the support of research through project grants versus broad institutional grants, and how to wed the cultivation of the best science to the training of enough scientists, broadly distributed throughout the country. Even more fundamental and serious is the failure of the university and the scientific community to effectively communicate its values, its purposes, and its contributions to the public and to the lawmakers.

Although these and other problems connected with federal support of academic research could be alleviated by increased funds, it is unlikely that there will ever be enough funds to satisfy all legitimate requests. In short, we had better face up to the underlying problems. With the increasing size of the academic science establishment and the proliferation in the number of promising avenues of research, failure to develop a coherent approach could bring even greater pain at a later date should the enterprise suffer a loss of public confidence and support.

As we move to unite the knots in the existing policies and arrangements for federal support of academic research, we must, I believe, find a healthy accommodation between a laissez-faire system and centralized control. Forces in the direction of detailed planning of basic research and graduate education have been resisted because of the inherent unpredictability of the results of scientific research and the needs of our society, and because of difficulties in estimating the long-term national requirements for scientists and engineers. While I agree that central direction of federal support of academic science is not conducive to the maintenance of vigorous, high-quality academic research, neither is chaos. Nor can we entirely capitulate to the vested self-interests in subgroups of the scientific community that will resist any change or trade-off that they believe would threaten their interests. What I am suggesting is a better articulated framework for federal support of science and an indicative plan, looking a few years into the future, that will provide a general guide for the allocation of funds, at least, and provide a necessary degree of stability and predictability for future planning by the universities and the government agencies involved.

There are many ways in which the federal support of academic science can be carried out—different mixes of government agencies and universities, as well as different mechanisms for the support of research and for the support of graduate training. What may make sense at one level of consideration may not make sense at another level. I believe that we do not know enough about the interrelationships of the various parts of the scientific enterprise, the various types of support, and the various objects of support to construct a comprehensive blueprint or plan for proceeding. However, I am convinced that we need to sharpen our analytical tools and capabilities, identify and acquire the necessary data, devise working hypotheses, and be willing to experiment with subaggregates of the system so that we will be in a steadily improving position to deal effectively with the entire set of problems. And we will have to move further toward the generation of broad-scale, long-range plans.

This problem can be likened to the continued, healthy growth of a delicate and complex organism. It is not analogous to the stages of human growth from childhood to adolescence, adulthood, and old age—and I hope the latter is not in sight. Rather, it is more like the problems of medicine and physiology, where we understand some of the pieces, but where our understandings are isolated and do not explain the functioning of the organism as a whole. The pieces I refer to are basic research, education, applications, and their coupling to technology. Our job is to make the organism healthier—not just its component organs.

Impact of the National Scientific Effort on Social and Economic Progress

A question just as fundamental as that posed by academic science concerns the coupling between the national scientific effort and our country's social and economic progress.

During the past 2 years I have been deeply involved in two studies of the so-called "technological gap" issue. One was carried out at my direction within the U.S. government. The other was undertaken by the Organization for Economic Cooperation and Development, in Paris, in preparation for the OECD Ministers of Science meeting last March. The analysis of technological disparities among industrially advanced countries and their basic causes makes it clear that the United States does better than most countries in harnessing science and technology to economic and social progress.

Europeans tend to regard the technological gap as a new phenomenon, and in doing so overlook the long history of U.S. preoccupation with industrial growth. There was considerable debate on this issue among the "founding fathers" after the American Revolution. According to George Soule, in his *Economic Forces in American History* (*2*), Thomas Jefferson favored a nation of landowners, principally engaged in farming, to avoid the poverty and exploitation of the working classes which accompanied the beginning of the industrial revolution in England. In this debate, Alexander Hamilton's differing views prevailed, and Hamilton should be credited for the strategy America used to overcome its technological dependence on Europe. The basic elements of this strategy, reflected in Hamilton's "Report on the Subject of Manufactures," submitted to Congress in 1791 when he was Secretary of the Treasury, were the protection of "infant industries." Perhaps more importantly, he urged the promotion of immigration of technologically skilled manpower and the encouragement of capital inflow from abroad. Since that early time, numerous European observers, from Alexis de Tocqueville on, have commented on the positive American attitudes toward technological change and the introduction of new technology in industry.

Federal policies and programs aimed at stimulating American industrial technology, directly or indirectly, are simply the modern version of Hamilton's infant-industry argument. What makes it more difficult now is that we are trying to strike a balance between a national view and a world view. In Hamilton's day, government policies toward satisfying the needs of 10 million people couldn't upset any international applecarts. Today, the currency and the military power of the United States are dominant forces in the world of commerce and international order. Government policies with short-term domestic objectives can, through international repercussions, have longer-term adverse effects on both the international and the domestic scene—witness the run on the dollar due to what others regard as overexpansion of domestic programs.

Despite the acknowledged American success in most fields of science and technology, there are some industrial people in the United States who feel that the effect of our emphasis on academic science has been to draw off too many talented people from other creative functions of society, such as industrial engineering and innovation. They feel, for example, that contemporary engineering training is not appropriate to the conduct of engineering in industry—although others dispute this allegation.

Another difference of view concerns the degree of coupling of the results of government-financed research and development, particularly in the military and space areas, with the needs of civilian industry. Again, some will allege that the federally financed research and development effort has siphoned off or otherwise deprived industry of creative talents that could be put to use in commercial R&D—that it has undesirably inflated the salaries of scientists and engineers employed in nongovernmental commercial business.

With regard to the "spin-off" question, almost everyone who has looked at the evidence agrees that the exploratory development programs of the Defense Department and NASA have enabled us to press the technical arts to their farthest limits. Some of our best, newest, and most thriving industries have their roots in this government-financed industrial activity. Our favorable export balances largely reflect export of products born of intensive technological effort in industrial sectors such as sophisticated electronics, computers, and aircraft, which owe much to the stimulation of federal support. But we remain unclear about the diverse effects of federal support of research and development in the aerospace and electronics industries on our industrial base as a whole. The dual fads of enthusiasm and complaint about "spin-off" are not likely to be dissipated without further intensive study of the complicated cause-and-effect relationships observed over a considerable period.

One could advance the hypothesis that, in a sense, technology per se is to industrial innovation what science is to the generation of new technology—that the general search for new technology is the industrial equivalent of basic research. In my view, though, there is an important difference between science and undirected technology. The best basic research is directed at carefully conceived problems framed by the investigator. I question whether government programs aimed at the general development of new technology would be effective in advancing civilian-directed industry. On the other hand, technology which is a product of industrial R&D contracts aimed at satisfying the exacting requirements of military and space systems—requirements which go well beyond civilian needs and which set concrete performance goals for the product—is more likely to be applicable. We have just witnessed a magnificent demonstration of this point in the Apollo 8 mission, in which the huge Saturn V had to perform flawlessly on its first flight, as did computers, a far-flung communications and tracking system, and a complex human organization—not to mention the astronauts themselves. This distinction between general technological development and the achievement of measurable goals was not well brought out in the OECD studies and seems to have been blurred in some foreign debates on government programs for strengthening the technological base of industry—say, the computer industry.

It should also be observed that technological development is enormously expensive as compared to most basic research, and that, although the Department of Defense, NASA, and the Atomic Energy Commission, among others, do support exploratory development efforts, development can normally be supported as a federal expenditure only where it is aimed at specific needs that the public, expressing itself through the Congress, regards as commensurate with the investment. This, of course, raises the $64 question of the appropriate role of the U.S. government in supporting or promoting research and development for the prime purpose of advancing industrial development and growth for civilian ends.

Although there is general satisfaction with the health of American industry and its rate of technological innovation, there are some areas (environmental pollution is an example) where the ordinary market rewards do not stimulate industry to develop at an adequate pace the new products and processes needed by the general public. In the field of air pollution there is a lack of strong private incentives, and the urgent need for improvements in pollution-abatement technology have called for government leadership.

The leadership for pollution abatement, as present, lies in the government through its role in standard setting and in supporting science and technology to demonstrate what can be done, and how. It will be the essential job of industry to find cheaper and improved ways of applying the new technology. In the longer run, this is bound to lead to an increase in private activity and a lessening of the financial burden on the government.

Government standard-setting has been an important indirect means of stimulating industrial incentives and competition to improve the quality of products affecting other aspects of the general health and welfare. Through food and drug legislation we have been able to maintain high standards of drug safety and efficacy. Automobile safety standards are another example. Within the interval of a few years we have seen a dramatic shift in the attitude of the automobile industry from a phobia about mentioning automobile safety in advertising to today's promotion of safety features in meeting industry competition.

A great deal more work needs to be done to sharpen the tool of standard-setting as a means of introducing product improvement and change in particular sectors of industry. Standards must be based on sound scientific evidence, which must be continuously reexamined and improved. They must be set with regard to the industry's economic, managerial, and physical ability to respond. If there is careful regard for the sensitive interaction between incentives for innovation and requirements for protection, government standard-setting can exert a strong motive force for private investment.

At the same time, when looking to industry one should be realistic about the size of the market incentives needed to stimulate private investment. The expected market demand or dollar sales volume must be large in relation to the R&D investment that can be justified to produce the improved product.

In some areas it will be necessary for the government to directly stimulate industrial innovation in important but lagging industries. In some cases it can do this as a consumer of a large number of units (such as military housing) or through partial or full support of research, development, and demonstration projects.

The question of whether there is need for an overall governmental policy for strengthening civilian technology generally has been held in abeyance. In the absence of a direct interest in a specific industry or social problem, the government has not adopted, as a general approach, direct measures for encouraging industrial invention and innovation per se. Patent and tax incentives have long provided indirect encouragement for private investment. With the exception of selected industries somehow identified with the public interest (for example, agriculture, atomic energy, the supersonic transport, water desalting, pollution abatement, and a few others), the government has not subsidized civilian-oriented industrial research. Further measures to stimulate technological innovation may be needed, but there appears to be no need for an across-the-board, direct approach by the federal government. Nonetheless, we should watch closely the experience of Canada, the United Kingdom, and France in their new programs for subsidizing the development of new civilian technology, to see whether experiments along this line are indicated for the United States.

Government Science Organization

Thus far I have dealt with some of the issues that academic science, industrial research, and social needs pose for U.S. science policy. The fourth question asked by the OECD examiners is even more elusive: how adequate is the organization of the federal government for dealing wtih these questions—particularly, how adequate is the organization at the Presidential level?

I believe we have the right basic ingredients. The Office of Science and Technology has grown steadily; it now has a staff of over 50, more than 20 of them professionals. This high-quality staff works closely with the agencies, with the Bureau of the Budget, with the National Security Council staff, with the Council of Economic Advisers, with the White House staff, and with the committees of the Congress. Its central concern is the evaluation of existing and potential programs, the coordination of agency programs, and participation in the larger discussions of priorities and emphases. On selected major issues it benefits from the external advice of the President's Science Advisory Committee and over 200 consultants. Internally it draws on the expertise and experience in the agencies through the Federal Council for Science and Technology and its panels.

But I believe OST and the Science Advisory apparatus need strengthening. Before I get more specific, I would like to caution that the easy answer to all problems in government, scientific and nonscientific, seems to be to move them closer to the President. I do not think that answer is tenable for many things —he is already overburdened.

My first guiding principle as regards government science organization (and most other organization) is this: decision-making should be pushed to the lowest responsible level appropriate to that decision. I question the wisdom, for example, of asking a high-level group to make decisions which could be made by a laboratory director. On the other hand, there is an important class of problems that involve general questions. In my view, the more general the question is, the more it should approach the center of the decision-making apparatus. For example, one function that can best be performed at the center is overall planning. Today we are facing a set of problems involving science and technology, and their interaction with many institutions and sectors of our society whose dimensions extend well beyond the capabilities or jurisdiction of any single department or agency of the federal government. I believe that the development of a greatly improved capability to analyze these complex problems and to foresee their eventual impact on society will be an important step in the evolution of the science organization at the presidential level. Such analysis must be carried out without the initial constraints of agency jurisdiction, and in intimate relationship with the decision and policy-making processes in the Executive Office of the President.

It is clear that we need more systems analysis on a government-wide scale. I do not mean the formal and sometimes sterile approach of professional systems analysts. Rather, I refer to analysis that is both tied to the decision-making function and involves the creative thinking of a large number of people looking at the inventive process, without undue concentration on the techniques and methodology of systems analysis.

There are many basic questions facing the government that we have been unable to analyze in a systematic way —questions like these: How many graduate schools, of what kind, does the country need to meet its present and future needs? What is the effect of the various development programs on the future requirements for research support? What trade-offs were we really making when we initiated a space program, and what trade-offs will we be making if we cut it back now? We need studies like these as part of a continuing assessment program. The Office of Science and Technology could eventually evolve into an office of planning, evaluation, and analysis, looking broadly at national problems with some scientific or technological component but extending well beyond the purely technical areas. The OST has been moving in this direction in its work in environmental pollution, urban needs, and the world food problem.

In fact, OST has a newly formed Office of Energy Policy Coordination which is undertaking a broad study of the many important energy policy questions that affect more than one agency of government. The new policy questions are occasioned by the rapid advance of nuclear power on the energy scene; the need to reconcile air-pollution and water-pollution programs with our demand for low-cost energy; the growing demand for energy in relation to available supplies at economic prices, especially supplies of natural gas; the basic question of future import policy concerning oil, gas, and uranium; government policy toward developing sources of oil and gas from shale and coal to supplement natural supplies; and many others.

As I have indicated, an essential feature of such studies is that they be carried out in such a way that they are an integral part of the policy-making process; that they deal with the real world economic and political constraints, without accepting them as immutable.

Such an evaluation capability should be part of the forward planning function that needs more explicit recognition in the Executive Office of the President. By "planning" I do not mean a rigid blueprinting of the future. Rather, I mean a best current projection of the future, and of alternative futures, based on present activities and planned new ones. We simply are not smart enough to put together large-scale plans for many things at the present time. However, by developing a capability for analysis, it should be possible more and more to chart the future analytically rather than through mere intuition and debate.

There are some things that cannot be done without large-scale planning. A national telephone system required an overall plan, and systems analysis and engineering were needed to put it together. We have undoubtedly foregone some competition in the process, and perhaps some of the components are more expensive than need be, but the need to eliminate internal incompatibilities was overriding. Similarly, despite the political fragmentation of many communities, water and sewer systems must be put together according to a plan. For large-weapon or space-systems development, the complexity of the many efforts which must jell, with a lead time of 5 years or more, requires a working plan. Many more big national problems are forcing us in this direction. The structure of university science may well be approaching that divide where the need for overall systems planning will take precedence over the goal of obtaining maximum health of each of the parts taken one at a time.

Undoubtedly, we will have to face up to the need for more comprehensive planning. We can begin—in fact we already have begun—to isolate those manageable pieces of the larger problems that lend themselves to analysis, and, as further areas yield to analysis and we better understand the boundary conditions, it should be increasingly possible to predict likely outcomes from given actions.

Of course, as scientists we recognize that the best of analysis cannot predict the outcome if we do not know the relevant inputs, or, as is so often the case in complex problems, when we are not even sure that we know what all the relevant variables are. In such situations we rely on the carefully controlled and evaluated experiment. The experiment is the lifeblood of science, and we must learn to use it effectively in other areas. For example, in dealing with urban problems we must learn to employ experiments to help answer the larger questions that do not yield to analysis.

We shall have to foster many experiments involving large systems, but naturally we need to know how we will evaluate them when they are finished. Rational analysis coupled with experimentation should make it clearer what we need to do by experiment and what choices are available through analysis. Unfortunately, we have too often substituted bureaucratic and political processes for either rational planning or experimentation. In a democracy this may always be the case, but the analysis will at least provide a better basis for political discussion.

A second principle of government policy ought to be to maintain competition. Insofar as government actions and organization are concerned, many people now suggest a highly planned economy for science, with a rigid separation of functions and a careful elimination of duplication. Our successful experience suggests a contrary course. Most government agencies that have remained virile and avoided deterioration have done so, in part, by stepping on each other's feet. As a general rule, if there is a large opportunity or need at stake, it is profitable and appropriate to employ both competition and careful planning.

More importantly, basic science is both a cooperative and a highly competitive activity. Its progress depends on the stimulation provided by competition. For a vivid illustration I refer you to James Watson's fascinating book *The Double Helix* (*3*). In science, as in economic processes, competition stimulates the quality of performance and must be fostered, together with the cooperation which comes through an open, widespread, and effective communication system among scientists.

Proposals

Finally, I want to make some specific proposals.

First, I believe the Office of the Science Advisor needs strengthening, not only through more staff capability for analysis and planning but through the addition of full-time top-level people. In short, I propose that the Science Advisor be made the head of a three- to five-man Council of Scientific and Technical Advisors. My reason is simple—the range of matters he must at present consider is so broad and his responsibilities are so extensive that he needs help. Alternatively, one might add three assistant directors to the present director and deputy director.

I also believe that, provided the staff resources were available, it would be wise to ask such a council to submit to the President and the Congress an annual report on the state of U.S. science and technology, roughly analogous to the annual Economic Report.

Second, we should reexamine a possibility we put aside some years ago—namely, that those scientific activities not tied to the central purposes of an agency be considered for inclusion in a department of science, with the National Science Foundation as a core. Science has now assumed such importance to the nation that its position would be stronger if it had a voice at the Cabinet table.

However, in making that proposal I want to make it clear that I would *not* consider concentrating *all* of our science activities into a central agency. A strength of the American establishment is the realization that *science is part of everything*. Those research activities which are integral to a department's mission or which form the basis for its future should be left where they are. More than that, agencies should be encouraged to strengthen their research and development base. But there are other scientific activities of agencies which may be somewhat peripheral to the main job of an agency but are nonetheless important, and these would flourish if transferred to a department of science.

In determining the organizational elements of a department of science, thought will have to be given to the department's relationship to advanced education on the one hand and technological advance on the other. The more the department is oriented toward new technology, the less it is equipped to deal with academic science and advanced education, including the humanities. The more it is oriented toward basic research and academic science, the more it is fitted for a broader role in higher education. On this score, one could invent several cuts that would represent an improvement over the present situation, but I am far from sure what the best cut would be. My present feeling, though, is that the critical questions concern basic research and higher education, and that technological development is more appropriately conducted by agencies with specific tasks and missions.

In the power equation of Washington, such a department of science, if it is to be influential, should have a budget of $2 billion or more. Its principal officer would have line responsibility and public accountability and, most importantly, the interest and confidence of the President, the attention of the Bureau of the Budget, and the ear of the Congress.

With a strong cabinet officer for science in the Executive Branch, there would automatically be a strong congressional counterpart committee having a broad interest in the problems of science and technology, not a minor or incidental interest. We already have committees like the Joint Committee on Atomic Energy and the House Science and Astronautics Committee that are broadly educated in particular spheres of scientific and technological activities, and I am confident we could have committees of this caliber to supervise this department too.

Conclusion

Both the problems and opportunities facing government science policies loom larger than ever before us. I have been privileged to have had a part in setting U.S. science policy and am proud of what has been accomplished so far.

Despite the last 25 years' evolution of the U.S. science structure in the U.S. government, we are still in the early stages of learning how to realize the potential of science and technology for the national good. But we have built a strong foundation, on which further additions and structural changes can be made wtih confidence.

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Science and Social Purpose

Proposed fundamental changes in the national science effort are discussed in terms of biomedical research.

James A. Shannon

Discussions of contemporary science too often focus on the painful and disruptive effects of a reduction in federal support—an inevitable consequence of general constraints on federal expenditures. They are less than helpful in the broad analysis of the general support system itself.

It would be well to acknowledge that there are fundamental imperfections in present federal mechanisms for the support of science, and that the ultimate patrons of science, the public, have not been given an understanding of science that can serve as a base for its continued support and evolution. A simple return to larger funding of research would mitigate some of the immediately urgent problems, but this alone would not adequately serve the long-term needs of science. Here I explore the basis for this conviction, as well as its implications for evolution of science policy.

The urgent tasks that now confront the scientific community, though not simple, are quite clear.

1) The scientific community must adjust itself to less than optimum funding, at least for the present, while retaining the essential strength of the scientific enterprise.

2) It must seek out the imperfections of the present support systems, and propose modifications that are corrective and, in addition, rationally based and generally applicable to the diverse fields of science.

3) Finally, the scientific community must devise means of fostering a broader understanding of the revolutionary technological forces that can be unleashed by a vigorous science for the betterment of society.

All three of these tasks are feasible, each is urgent, and each will require a high degree of scientific statesmanship.

General Considerations

Science has flourished remarkably in the United States since the end of World War II, largely as the result of intelligent use of the vast sums of public money available for a wide diversity of scientific and technological activities. The government policies which fostered this development emphasized the promise of science for the attainment of major public objectives. These policies were pursued even though only a few of the individuals directly involved in the political process truly understood the difficulties inherent in the problems that scientists were asked to solve, or the character, complexity, and *modus operandi* of science. Further, as diverse fields of science rapidly evolved under these circumstances, the scientific community made little attempt to increase public understanding of these characteristics of science, or to establish the necessary coupling between the satisfaction of social needs and aspirations on the one hand and broad support of research on the other.

For a time, science seemed to be isolated from the real world and its problems. The public attitudes which fostered the outpouring of support were a popular expression of faith in the ultimate power of science to benefit mankind. Many scientists, on the other hand, viewed activities in their own fields as a type of pure intellectualism —an expression of what is best in our society, not necessarily connected with public needs and problems or social purposes. Such a view is reasonable for the individual scientist but does not provide an adequate base for broad public support of a more general enterprise.

It is true that much of science was defended before the public by hardheaded and sophisticated administrators. They were convinced that science could, if properly supported, make broad contributions to society, and their plans, approaches, and public attitudes reflected a high degree of realism. These attitudes prevailed in the programs for the exploitation of nuclear energy, in those for the development of new weapons, and, to a large extent, in those aimed at alleviating disease and disability.

The coupling of research with broad social issues was less well articulated in the development of support programs for basic research, or for the "fundamental research" essential in scientifically based missions. This latter type of activity, frequently called "mission-oriented basic or fundamental research," was deemed essential to an agency's mission when this was viewed broadly and with a concern for the future. However, too often the activity was buried within a complex agency budget and not presented as an essential part of a rationally evolving program. The opportunity was missed

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to couple fundamental research with applications and developmental activities, particularly as these related to the general social purposes of the agency.

For many areas of science (medicine, perhaps, is an exception), the major impetus for expansion was external to science as such. It was a response to deficiencies in U.S. programs perceived when other nations made striking technological advances that had implications for the defense of this nation, or that generated urgent, but poorly defined, concern for national prestige. An example is the sizable influence that Sputnik I and the subsequent evolution of the Russian space program had on federal spending for research and development. This event did more than change the order of magnitude of U.S. R & D expenditures for defense and space; it had an influence on all areas of R & D. In fact, by precipitating the Office of Education into the mainstream of higher education through enactment of the National Defense Education Act, it may well have changed the course of higher education in this country. In any case, the burgeoning economy of the United States, with its already broad technological base, imposed few serious budgetary restrictions on science-program development during the late 1950's and early 1960's.

This set of circumstances permitted science in the United States to grow more or less in accordance with its own internal logic, being guided more by considerations of excellence, productivity, and freedom of individual effort than by consideration of the extent to which it might satisfy definable social needs. It seems likely that the period 1945–1965, particularly the last decade, will be viewed in retrospect as the time when U.S. science reached the summit of broad uncritical public support—what might be called the "Augustan era" of American science. But this was not a planned "happening"; it was more an accident, or spin-off from an affluent society's making bountiful contributions to science for diverse and often vague purposes. Fortunately, these contributions were, in the main, managed intelligently.

Such a situation, anomalous as it appears, in retrospect, to have been, led to the evolution of programs that were a mix of basic, applied, and developmental activities. As the broad program evolved, its continued support and growth depended directly on obvious concurrent benefits as well as on expectations for the future. These benefits were derived largely from the applied and developmental portions of the activity, rather than from the basic science involved. Such practical benefits dominated most presentations of accomplishments in all areas of science. Meanwhile, the public comprehension of research and development was shaped by mass-media information techniques which presented the progress of science as a stochastic series of exciting science spectaculars, without giving any sense of the fabric of scientific continuity and of its underlying warp and woof.

One wonders what the public attitudes toward science would be today if more attention had been devoted, during the past two decades, to education of the public in the internal complexities of science, and in the relationship between scientific discovery and technological advance. One should not lightly dismiss the role that presentation of the adventure of scientific discovery can play in motivating the public to support science. But it is important for scientists to understand that the motivating forces that captured public interest a decade ago have little relevance today. If science is to remain healthy and vigorous and is to continue to advance, a more rational basis for development of the national science effort must be found.

Despite the anomalies, the nation has acquired a broad and vigorous base in most general areas of science. During the present period of fiscal constraints, this base can provide a sound point of departure for the next stage in the exploitation of the nation's intellectual resources in science and technology. In the meantime, we must correct the fundamental weaknesses in the support structure, weaknesess that can place our long-term scientific prospects in jeopardy.

I am firmly convinced that it is possible to improve our present support mechanism for science, and to provide for a more rational distribution of supports without hampering the productive activity now in being. I am also convinced that such action must be accompanied by a coupling of activities aimed at the acquisition of new knowledge and activities aimed at applying that knowledge for the attainment of social objectives.

Because the changes required will involve a sharp departure from the past, such a development will require very thoughtful planning. Much is at stake, and there is no precedent or established design to guide us.

Before considering how our national science effort can be made more effective, one must clearly understand the distribution and magnitude of our current scientific effort and the critical strengths and deficiencies of present support mechanisms, and have some perception of the social needs that will provide the ultimate gauge of relevance and progress. Such a mix of substantive, policy, and procedural considerations is not amenable to simple treatment—certainly not if one attempts to consider science and its usefulness as a whole. However, it is possible to examine a major segment of science in these terms and later review the results for their relevance to all fields of science. Such considerations could then provide a basis for designing overall national policies.

The discussion of the biomedical sciences which follows is not such a definitive analysis. It is, rather, a series of reflections on some of the more important issues. Furthermore, I have selected the biomedical-science area for comment more because of my acquaintance with that area than because of a judgment on its relative importance.

Biomedical Research—1968

The striking World War II advances in medicine, a progressive public understanding of the socioeconomic burden of chronic illness, and our ignorance in relevant areas of science led to a general acceptance of the view that the ultimate resolution of major disease problems was possible only through research and the acquisition of wholly new knowledge—fundamental as well as applied. These views led to the enactment, during the late 1940's, of the landmark legislation which was the basis for the development of the modern NIH programs for the support of biomedical research. However, during these initial stages of the organic growth of NIH and other federal programs, the complexity of the biomedical problems and the proper scale of an effort that would satisfy the needs were matters not seriously considered or generally discussed.

The longer-term aspects of furthering medical capability were first presented formally by NIH to the Department of Health, Education, and Welfare in the summer of 1955, in a series of discussions with the then Secretary, Marion Folsom. This resulted in general agreement on the need to expand broadly the research support programs of NIH, the pace to be determined by the availability of scientific resources rather than by any specific limitation on dollars. It was also agreed that the existing science base was inadequate for the major effort to be applied and for developmental programs directly targeted on the clearly visible great medical problems. It was also apparent that the available manpower and facilities were insufficient for exploiting the scientific opportunities or for mounting an attack commensurate with the seriousness of the problems posed by disease.

Major expansion of fellowship and graduate education programs, designed to produce scientists rather than medical specialists, was proposed and approved. A precedent-breaking program of federal assistance for the construction of medical educational and research facilities was presented, but this program fell afoul of the then strong congressional opposition to federal entry into education, and the lack of broad support from the academic sector. When finally enacted, in 1956, this legislation decoupled, for support purposes, science from professional education and provided for the construction of research facilities only.

Secretary Folsom was responsible for another action of far-reaching consequence. He commissioned a committee to inquire into many of the important issues confronting biomedical research. This gave rise to a report, commonly called the "Bayne-Jones Report" (*1*), which, together with a later report commissioned by the Senate Appropriations Committee, the so-called "Jones Report" (*2*), provided much of the basis for the vigorous but rational support of the biomedical sciences by congressional leaders. These two reports also provided the philosophical and practical basis for an attempt by Folsom's successor, Secretary Flemming, to redress some of the imbalances, within institutions, being generated by the "project grant" as the sole instrument of federal support in the expansion of biomedical research and related training. Secretary Flemming was successful in obtaining from Congress an amendment to the Public Health Service act authorizing the award of "grants for the general support of research and research training programs" (*3*) of institutions. This was the origin of the institutional support programs of NIH, represented by NIH general research support grants, biomedical science support grants, and health science advancement awards.

The events of the late 1950's are of special interest as we search out the origins of deficiencies in the present project-grant system of research support. It was proposed initially that a substantial portion of the total federal support of research should be general support. In the first year, 5 percent of the total budgeted research grants to be made would be in the form of general research and training grants. This was to increase to 10 percent in the second year and 15 percent in the third year. An additional 2 to 3 years, it was believed, would be needed for a definitive study of the effectiveness of the program. This study would provide the basis for determining what proportion of total grant funds should be made available through project grants and what proportion through general grants. The latter, it was suggested, might well constitute 25 to 30 percent of the total. Unfortunately these proposals found little merit in the eyes of the individual scientist and his immediate supporters, since it appeared that they would diminish the share of resources available to him and his field.

Furthermore, attitudes toward such concepts of funding were affected by the trend toward focusing of popular interest and attention on specific achievements. For example, in the field of cardiovascular medicine it has been more convenient to view research progress in terms of the progression from "blue-baby" operations, through complex vascular surgery and open-heart surgery, to, finally, heart transplantation than to consider the vast scope of the interrelated basic scientific effort that necessarily preceded each of these achievements. And it has been in the past, and indeed still is, simpler to raise funds for quite explicit programs which tend to be short-term, such as the testing of a specific drug, than for longer-range and more complex studies that are more general in nature but are necessary if substantial advance is to be achieved. The project system tends to foster continued emphasis on short-term prospects and on individual science spectaculars. These circumstances lead to an environment within which the scientist can expect to be asked, much to his consternation, "What have you done for me lately?"

In such an environment, and in light of the traditional distaste for federal intervention in the educational process, it is not surprising that medical schools delayed asserting even a modest need for federal support for their basic educational programs until the early 1960's. It is only during the past year that the medical schools and the medical profession have agreed that massive support is essential for both current and expected educational programs if these institutions are to meet the broad social objectives that society has placed before them. Similarly, only recently has serious attention been given to the general needs of university-based science and education. The translation of these needs into fully realistic federal programs and appropriations has yet to be achieved. These perceptions of need come, unfortunately, at a time of heavy demands on the federal budget, associated with broad social turmoil, rapidly mounting federal costs for education and R & D in general, and enervating international commitments.

I should emphasize at this point that there is indeed an imbalance between support of research and support of education in our professional and graduate schools, and that there are broad deficiencies in both the educational and the socially oriented service functions of these institutions. The genesis of the problems, however, is not the development of a massive federally supported research activity, as is frequently alleged, but, rather, the long delay in recognizing, and in gaining consensus on, the parallel role the federal establishment should play in the progressive evolution of broad educational programs and socially oriented service programs. This role has not yet been comprehensively defined.

But for all these deficiencies of the support system, a highly diversified biomedical research activity has been developed. This is widely dispersed across the nation and is generally characterized by excellence. Its major weaknesses stem from the support of research alone in a situation in which research, education, and service are intimately mixed, and from the almost exclusive use of project systems of support by all agencies. These two characteristics of the support system have resulted in a fundamental instability of institutions of higher education at the very time that new and broad educational and social functions are being imposed upon them.

Remedial Action

I turn now to consideration of what I believe must be done to provide a solid base for the further development of the biomedical sciences. Clearly, whatever is planned must be planned in relation to the general problems of education and institutional development. Note must also be taken of the pressing service-related activities of many of the institutions involved.

I do not present any detailed arguments, only a few broad generalizations. I trust these will be viewed by some as informed judgments, since I know they will be taken by many as a statement of personal prejudices.

Institutional support. To meet the needs and correct the deficiencies of the complex programs I am discussing, substantial funds must be made available directly to institutions of higher education for general support of their basic graduate and professional educational functions. These funds must be adequate, and must be made available by mechanisms which permit the institution as a whole to grow and to attain general educational competence as well as the greatest possible degree of excellence. Further, these objectives must be attained within a system of support that gives the federal sponsor assurance that the broad public objectives for which the funds are made available are indeed being well served.

If the federal establishment provides this type of funding, the amounts will be substantial. This in turn will impose on the university, in the areas of graduate and medical education, wholly new obligations. The universities and medical schools will have to indicate the size and scope of the central educational function, upon which their educational achievement will be judged. Further, methods will have to be developed for assessing the quality of the central educational enterprise that is supported. For example, medical schools that receive general support funds because of an urgent financial crisis in their funding must realize that this is possible for a year or two in an acute emergency but is not a normal or indeed an adequate base for long-term support, and certainly is not a rational basis for long-term development.

Given a more adequate and more stable financial base, institutions of higher education could plan their overall development in the light of the broad educational and social responsibilities they have recently acquired. Beyond this central core of support, the project system of grants and contracts can continue to provide the principal means for extension of mission-oriented research programs.

Support of mission-oriented research. Once the institutional integrity of institutions of higher education has been secured by general support programs, the mission-oriented agencies of the federal establishment can move more directly toward accomplishment of their special missions. They can be more free in selecting the institutions that are to receive support for research and development. Also, the terms and conditions of their awards can directly reflect the program needs of the agencies' objectives rather than a compromise between the mission needs of an agency and the sometimes overriding needs of higher education, as is now the case. With institutional integrity assured, the way begins to open for an enlarged and more sharply focused research activity, accompanied in many cases by a much greater measure of national organization than now exists (*4*).

Elements of such organized research, when it is performed within an academic environment, can enrich the academic environment. However, such activity will, I believe, be increasingly performed in research environments peculiarly devised for such complex but coherently related research undertakings, be these in universities, medical centers, research institutions, national laboratories, or industry. In this case, the further development of the undifferentiated base of the biomedical sciences will proceed in academic environments devised to provide the essential coupling of research and education, and will be supported as an objective apart from, but complementary to, mission programs.

Allocation of resources. Other requirements must be met if the mix of undertakings noted above is to be productive. The first requirement is a better information system, one capable of providing an ongoing analysis of the nature and extent of scientific effort in areas of direct relevance to broad problems in medicine and health. What is needed is not a system that provides for the simple storage and retrieval of documents or indeed of the data and other information they contain. Rather, the system must be capable of providing analyses and arrays of information specifically relevant to broad sets of problems perceived from an overall point of view (*5*).

The present informational systems of federal agencies may satisfy agency purposes, but they do not satisfy the broader national need. For example, NIH supports only about 40 percent of all biomedical research and about 55 percent of all biomedical research supported by the federal establishment. The rest is derived from other agencies—the Atomic Energy Commission, the Department of Defense, NASA, the National Science Foundation, the Veterans Administration, the Department of Agriculture, and other portions of the Public Health Service. There is not now any simple mechanism for analyzing all these activities insofar as they relate to the generalities of biomedical research. The analysis envisaged would not be a simple consideration of the biomedical sciences as such but, rather, would be an analysis of research and training in relation to the broader national objectives in the field of health. In this fashion science would assume its proper place as a competitor for the federal dollar.

Viewed in this light, research activity can be classified in very broad categories for central consideration of priorities in terms of social objectives. The allocation of resources then becomes manageable. One must accept the condition that such allocations must reflect a number of value judgments and are not amenable to simple linear scaling.

Central consideration of the use of science and technology in the promotion of health would be paralleled by central consideration of their use in relation to defense; space; resources, including energy and minerals; food; civil needs, including environment, housing, transportation, and many problems of our cities; and, finally, the knowledge base and general education.

One cannot hold a brief for any high degree of specificity or precision at this stage of development of a central program analysis and planning activity. One must recognize that our political system now makes resource allocations for science that are quite explicit, but does so by a series of judgments made in relative isolation from each other. It does not seem very bold to say that this decision process can be improved, and that allocations can be made among science areas, with some consideration given to the probable value of science to society. Since allocations to individual areas of science can never be absolute in the absence of unlimited resources, the allocation process must permit comparative assessment of competing fields. Finally, for broad acceptance by the public, the allocation process must provide input not only from science, the generator of new knowledge, but also from technology, the applier of new knowledge, and from the consumer, the user of technological applications.

Such a proposal is tantamount to suggesting the designation of a series of cognizant agencies for information assembly and analysis. These would not reflect departmental or agency operational structure.

Some central apparatus. However, for effective utilization of the organized flow of information produced by such cognizant agencies, such information would have to be collated at a high point in the Executive Branch, a point at which the critical policy and allocation decisions that would influence program development in science and education, and in the use of science for other social purposes, would be made. These decisions are so important that the level for collation of information could be no lower than that at which the National Security Council and the Council of Economic Advisors operate.

With a suitable central apparatus it might be possible to diminish the present chaotic competition for research and development funds among the major areas of scientific endeavor—the competition between the needs of research and education—and to consider these needs in relation to broad social needs and national purposes. The evolution of an increasingly firm sense of national capabilities and priorities would permit clearer expression of our national purposes in the pursuit and utilization of new knowledge.

I fully realize that we now have many central mechanisms for program review and policy advice, but, without considering each one in detail, I would hold that no one of them, nor indeed a combination of all, is adequate for our future needs and purposes.

General Prospects and Problems

But to return to the future of the biomedical sciences, the sequence of thoughts that I would like to leave with you is as follows.

1) The socioeconomic burden of disease is inordinate.

2) The economic cost, the most direct indicator of which is the unit cost of medical care, continues to rise geometrically with time.

3) The conquest of serious disease and attainment of the essentials for a better quality of life are not visionary goals. They will, however, require a substantial expansion of research under circumstances that provide comparably well-developed support for educational and service programs.

4) A prime essential for such accomplishments is the development of central analysis and planning functions that are adequate to the task of ordering national priorities and serve as a basis for the allocation of resources among broad fields of science and within the biomedical field.

5) There must be developed, in parallel with the expansion of research and the development of central analysis and planning functions, an adequate public information program that portrays not only achievement but also prospects and problems.

I would emphasize that each area of science has its own special problems. Biochemical science is no exception. It shares some of its problems with medical education and medical service.

These problems stem from a public awareness of our deficiencies in knowledge. The public has immediate experience of disease, disability, and death. Moreover, it has become exquisitely sensitive to certain deficiencies in our system of medical education. Such public knowledge, even though only partial, is too frequently the basis of emotional outpourings that result from nonavailability of physicians at times of medical need, or from individual failures of diagnosis and therapy.

Furthermore, members of the general community have reason to be dissatisfied with the results of scientific "tours de force" presented as scientific spectaculars but having little relevance to their own problems. They have seen new drugs produce defective children, and they have been told that the triumphs of molecular biology can lead to a social evil as well as to social good. They rightly care less about the niceties of bureaucratic structure than about the productivity of the total enterprise, and they have a right to have the fields of science, education, and service, as these relate to medicine, presented to them in a more unified and understandable fashion. They have a right to a more realistic presentation of the goals that members of the scientific community have set for themselves, and of the prospects of success, as well as a right to some conception of the mechanics of the process, including some appreciation of the projected time base. While they may not need to know more about the distribution of these activities within the academic and federal structures, they have a right to demand that bureaucratic considerations of departmental autonomy, institutional individuality, and freedom of the individual scientists will not, in themselves, impose barriers to the development of a sound science and the rapid translation of new knowledge into a readily available medical capability.

I am convinced that the trend of research, education, and service, as these relate to medicine, will, even more in the future than today, be the concern of the people who are consumers of the final product, and that this concern will increasingly be reflected in congressional attitudes. If this view is generally correct, then I would judge that, although there will be no riots in the streets, there could be generated high public pressures for change, which could be misguided.

I would hope that we can accomplish the necessary organizational and bureaucratic changes through rational processes within the scientific community and the branches of government rather than at the hands of a disenchanted public.

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MEDICAL CARE NEEDS IN THE COMING DECADE*

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BEFORE I begin the body of my paper, I should like to make a few brief comments. I have chosen to develop the economist's framework, perspective, and thought pattern. Though this may be unnecessary for many of you, I prefer to do so in order that we might better understand each other and so that we use a common language and understand each other's point of view. Only if this is done shall we be able to appreciate the significance and the limitations of the quantitative estimates that will be developed. Thus the "Introduction" that we might expect—this is a preface—is not really an introduction at all. It is an integral part of the paper. The second point I should like to make is that though we all recognize the importance of the question before us, we must not be too disappointed if we find that—given the quality of the data available at this time—it cannot be answered in definitive fashion.

These are the only points I need mention in the preface to the introduction. We can now move forward. If we are to discuss medical care needs in the coming decade and if we are to attempt to assess the quantitative dimensions involved, we must spend some time defining the terms that we use. It has become clear that those in the applied area —in this case medical care—and those who are concerned with and who determine public policy, as well as the statisticians, economists, and others who generate the data used by both groups understand the language they are using so that unnecessary confusion can be eliminated. Terms such as "needs" and "shortage" are used somewhat differently by persons in the various disciplines, and accordingly there has been more than the necessary confusion when the various disciplines have worked together. I should therefore like to begin by discussing with you some of the ways in which one might approach the words "needs" or "could use" or "demand." Let me do so in the context of public policy decision making. My reasons for casting this in the framework of public policy and decision making are fairly clear. Were medical care thought of as various other services are thought of, if medical care had the same characteristics that a variety of other services have, there would be little reason to discuss the question of needs in the coming decade. Indeed, under such conditions it is not even clear that we should use the term "need" (but more of that later) or examine the question that I am dis-

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cussing here. In that case one would assume that the supply of medical services would respond to the demand for them, and that demand and supply would be equilibrated through associated price changes—all this operating through and taking place in the market much as supply and demand responses work themselves out for a variety of other goods and services. Persons would choose to purchase the various medical services in quantities which they felt they wanted at the prevailing price, supply would respond, prices would change, demand would respond somewhat, supply would again change somewhat, and we should move to an equilibrium price in which the supply of medical services would equal the demand for services. As a consequence of changes in income, changes in tastes, changes in the price of other commodities, changes in the size of the population and its characteristics, and so forth (including changes in the cost of producing medical services as a result of changes in technology) the demand and supply characteristics would alter in time. As they altered there would be adjustments in price with tendencies toward an equilibrium price which would reflect the equality of the demand for and the supply of medical services. It is true, of course, that when the problem is viewed in a dynamic sense one may never reach that equilibrium. Instead, as we move toward it, it keeps moving away. In this sense—and of course for other reasons as well—this model is considerably simplified. Nonetheless this description of the operations of the competitive market would not be irrelevant—were medical care more like other goods and services.

Many of us would agree, however, that the characteristics of medical care are such as to suggest that the departures from the competitive market are many. Further, our attitudes toward medical care are such as to lead to an intervention whose purpose is to change, to alter, the outcomes that would prevail under conditions that did not include public decision making and intervention. This should not surprise us nor should it be deplored. It is true, of course, that many goods and services have special characteristics that impinge on supply, demand, and price. Market institutions do differ. I think that many of us agree, however, that medical care has a very large number of what might be termed special characteristics: for example, problems involving externalities, that is, situations in which persons who do not obtain medical care derive benefits from the fact that others do obtain it; problems of equity in distribution, that is, our view of medical care is such that our concern with an equitable distribution of services is far greater than is the case with other commodities; lack of knowledge on the part of consumers about the quality and effectiveness of medical care; the long period of time required for adjustments in the supply of personnel that render care, and so on. Some of these characteristics in and of themselves suggest the need for public intervention into the medical care scene. All of them taken together help explain why so many societies—even belatedly including our own—have involved government in medical care. It is more than a matter of infectious disease, epidemics, quarantine, and sanitation.

But if government is going to intervene, as it has on both the supply and demand side, it surely requires standards by which to judge the degree of intervention and the mechanisms for intervention. The demands upon resources, both budgetary and personnel, are many and strong. There are numerous things that the government can do with the resources at its disposal (obviously including turning them over to the private sector in the form, say, of a tax cut). Government, therefore, is confronted with the problem of choice much as individuals are. For however affluent we may feel, ours is nonetheless an economy of scarcity —all our wants cannot be met. Required, therefore, are criteria which can guide choice, criteria by which to choose wisely. It is in the development of criteria that one begins to feel the need for definitions of words such as medical care needs, or demands, or desires. If we want to use public intervention on the supply side in order to help the system respond we want to know what the system should respond to, what is our aim, what is our goal. Alternatively, if we want to assist individuals in their purchase of medical care by operating on the demand side, we not only want a model to judge the responsiveness of the system to changes in demand, but we also need some quantitative estimate that would enable us to answer better the question how much demand ought to be increased and what might we obtain as a result. For example, will given levels of demand help meet medical needs, help meet the demands that individuals have, or help meet individual tastes which may be less than or greater than their needs?

As you can see, my own interests lie in public policy. To estimate needs a decade hence simply because we are curious about them may be satisfying to some. But my own interests lie in what implications these projections have for policy. It follows, therefore, that I am concerned with the kinds of projections that are most meaningful to those who can affect policy. The projections, I argue, should be cast into a framework that is relevant to the criteria by which policy makers choose among alternatives.

In this context, then, I propose to discuss benefit-cost analysis as a useful input into the decision-making process. I do so for two reasons. First because it seems to me that this type of thought process ought to be understood by persons in the various applied areas (health, education, and so forth) since it is becoming more and more fundamental in government decision making. It would be unfortunate, for example, if leaders in the health arena were unfamiliar with the way persons in the Department of Health, Education, and Welfare and the Bureau of the Budget are beginning to think about medical matters. The consequences could be a sharp break in the ability to communicate, with obvious unfortunate results. But I discuss this also because it seems to me that it will help us understand some of the implications of alternative definitions (in particular, it will help provide us with a focus for our discussion of whether the word "need" appropriately belongs in the title of this paper).

It is abundantly clear that as individuals and as a society we cannot do all that we should like to do. We start with the fundamental fact—not assumption, but fact—that there is a scarcity of resources, that the problem we face is one of choice. Just as the individual faces constraints upon his own resources, be they money or time, so too with the nation. This constraint is well recognized and accepted—although at times we do try to avoid hard decisions by speaking as if our resources are limitless and choices were not necessary. Some, however, suppose that the constraint that applies is present when we speak of what the nation would like to do, and is not relevant when we consider what it needs to do. These persons argue that needs fall short of desires and that since needs are less and more limited, we can meet all needs, and that for that goal are not required to choose. In the case of food intake, for example, nutritional *needs* could presumably be met more readily—though with a less appetizing and interesting diet—than would be the case if we set a higher standard based on satisfactions. Housing standards, to illustrate further, might be much lower—and more readily achieved—were people to examine their absolute needs for shelter rather than to judge desires on the basis of their housing relative to that of others. I prefer not to engage in debate about the size of the absolute difference between physical needs and psychological desires. It seems to me that to talk about absolute needs not only ignores psychological considerations but is too great an abstraction from the real world. Questions of equity *are* important, and psychological satisfactions are no less relevant to public policy than scientific measures of need. The issue, after all, goes beyond the question of "needs" for physical survival. As an aside, it should also be pointed out that even issues of survival are difficult to judge, since many of the goods and services necessary for survival (for example kidney dialysis) cannot, except at great expense, be provided to everyone who may need them.

Let us for the time being, then, not debate the question of needs or desires. Let us recognize that we cannot provide for all of the needs that we may feel, though of course we can provide for some of them. The relevant issue involves the appropriate proportions. In recent months, for example, we have been hearing about the problem of choice in the context of "guns or butter." There are those who are convinced that the United States can have both guns and butter. Surely they are correct: a rich nation can have some—or even a lot—of both. But to pose the issue this way is to pose the incorrect issue. The real question is how many guns, how much butter? What is the trade off between them? What are the priorities? It remains a fact that the more guns, the less butter. Thus we require criteria by which to choose, a comparison of the real benefits of more of one or more of the other. So it is with all decisions about the allocation of resources. To put more resources (people, time, money, or effort) into one area is to put less into some other. If we are to choose wisely, we must try to assess what is accomplished with the resources when they are put into *A* as contrasted with *B*. We must consider the question—though it is difficult to answer—which policy or program increases total satisfaction the most—which gives us the greatest benefits?

This is a problem with which economists are quite familiar. Their study of a number of economic institutions has sensitized them to this type of question. Economists have long been involved with problems of budgeting—not primarily with where the last dollar was spent (a question of interest to accountants) but where the next one *should* be spent. The economist has traditionally asked questions about the use of resources (inputs and costs) and the product the resources create (outputs and benefits). He is accustomed to asking whether a given output has been achieved with minimum input (or, alternatively, whether an even greater output could be achieved with the same input). It is worth stressing the last clause: whether an even greater output could be achieved with the same inputs. An emphasis on the first clause—minimization of cost—unfortunately leads some to view economists as penny-pinchers, as if they were out to "save a buck" and as if this were some evil heartless thing. It should be remembered, however, that the purpose of saving a buck—of efficiency—is not to cut output, but to increase it because the bucks—the resources—that are saved are then available for use elsewhere (or even in expanding the same program). Thus the aim of minimizing costs per unit of output is the same as that of maximizing output per unit cost. Both make possible an increase in total satisfaction, in total utility. The aim of much of the analysis that goes under the headings of benefit cost or cost effectiveness is to find ways of obtaining more for a dollar—but not necessarily in order to spend fewer dollars. The purpose of the exercise is to find ways to maximize output, e.g., to help more sick people per unit of input, and so forth. If we ask whether more "good" could be achieved were alternative programs adopted, were alternative combinations of inputs used, it is in order to achieve more good, if possible—*not* in order to destroy existing programs. Put simply, cost-benefit analysis can combine two virtues: it enables one to be hardheaded and softhearted at the same time. Indeed, the softer the heart, perhaps the more hardheaded one ought to be.

It becomes clear that if we examine "needs" and the costs involved in meeting them and ultimately ask which needs should be met and at what price, we are asking about a comparison of the effectiveness and usefulness of various programs in relation to their costs. In determining public and private policy based on the effectiveness of alternative programs, effectiveness must mean the amount of good accomplished per unit of resource input.

As you can see, I feel the word need is intimately related to the concept of benefit. I find it difficult to define need except in terms of the benefits derived from meeting various objectives or, if you prefer, the costs of not meeting them. I confess that I should enjoy discussing with you in greater detail the methods in current use to try and measure the benefits of alternative courses of action, the benefits of meeting "needs." This is a subject which is of considerable interest to me both as a professional economist who recognizes the importance of the questions being asked and as a concerned citizen who is more than somewhat troubled about the answers being provided. To examine in detail the methodology of benefit-cost analysis, its strengths and its shortcomings, would, however, leave little time for the topic before us. I shall therefore restrain myself and spend only a few more minutes on the subject.

A number of consequences follow if we agree upon the need to compare programs—their outcomes and their costs. It becomes important to try to *measure* costs and benefits and thus to quantify where possible. This requires that there be essential agreement on the purpose, objective, or goal of the program, on the effect to which we look forward. This fundamental step would be extremely useful even were no further analysis undertaken. It may seem odd to stress this point but all of us, I am sure, are well aware that many a desirable program has been undertaken without specification of the results that one looks forward to and without agreement as to the end goal of the program. As a result there often are no criteria by which to judge success. Perhaps an example of such a situation is the result that we anticipate when we mount an effort to create a neighborhood health center. Are we to judge success by the number of people that are removed from poverty? Are we to judge success by the fact that we *offer* a community of poor people more medical care than they previously had available? Are we to judge success by the fact that people *avail* themselves of the care? Are we to judge success by the fact that the community takes an interest in the neighborhood health center and that this facility sparks community involvement and spirit? Is the goal of a neighborhood health center to make services available, to have services utilized, to make persons more satisfied, to minimize days lost from school or work, to improve health levels (and how do we measure these)? I think that you will agree with me that often in a variety of social programs there is little specification of the goals of the program.

But the problem goes well beyond the specification of the goals of the program. Even were these specified, we often are unable to determine whether the goals are being achieved because we have not been able to develop sufficient reliable and generally accepted indices. How do we measure health needs? How do we measure levels of health? Is the absence of illness a sufficient measure? If so, how do we measure the absence of illness? You will note the difficulty with using proxy measures. Some things are measurable while others are not. We must be careful to guard against the tendency to let this fact determine—either consciously or unconsciously—the aims of the program. It is far too easy to subvert the program by adjusting it to serve the proxy measures. If days of absence from school, for example, serve as a proxy for the illness of the child, we must be careful that the aim of achieving "success" does not lead to sending sick children to school. To extend this point a bit further, it is interesting to speculate whether a neighborhood health center would (in its initial phase) lead to higher or lower number of days lost from school. One can certainly develop alternative hypotheses, the one suggesting an increase, the other a decrease.

Thus far I have focused on some of the points associated with the specification of output and with its measurement. Since we are interested in *comparing* programs it also becomes necessary to find a common denominator, a way of translating the different outcomes and different benefits into a common unit of measure. Such a comparison is extremely difficult. This difficulty is found not only when we try to compare health programs with programs of education, with programs in natural resource, and so on across the broad spectrum of public and private investment programs. Such a common denominator is difficult to conceptualize even when we limit our review to a specific area such as health. A life saved is a life saved. But how does one life saved compare with the prevention of 1,000 cases of blindness? For that matter, is a life saved a life saved? Are the lives of the fetus, the infant, the child, the adult, the aged, the sick, the well, all equal? And if we relate lives saved to cost, how much are they worth?

These are not the easiest questions to articulate. The fact that they are distasteful often prevents our asking them explicitly. Nevertheless it cannot be denied that they are being answered all the time—whenever the level of appropriation for this or that program and the choice among programs is being determined. Indeed, since the questions are not asked explicitly, but are nonetheless being answered as decisions are being made, we have less debate on these issues than would be desirable. The consequences are policies that many of us, I assume, would consider odd if not perverse. The life of an airline passenger—as judged by how much we spend to prevent a death—seems to be worth more than the life of the person who could be saved by a program to make Papanicolaou smears more readily available. If we judge by decisions made in the past, the life of the individual who needs kidney dialysis is worth far more than the cure of the mentally retarded child. I leave it to you to decide where heart transplants would fit into the spectrum of therapy of even research budgets.

We have agreed that if we are to compare outputs we need some common denominator. One of the ways of equating outputs or benefits is to translate them into dollars. This is normally done by considering the implications of the program on the future earning power of the individuals who are affected by the program. Thus this approach is applied, for example, when the health activities add to the individual's future earning power by increasing his life expectancy or by removing his disability. In effect we say that the individual's productive contribution to society is increased as a consequence of the health program and that the increased productivity is measured by the increased income that he will receive. Thus a program that saves the life of a 30-year-old male yields approximately 35 years of productive contribution (65 years—the age of retirement—minus 30) and this productive contribution is measured by future lifetime income (all this with appropriate adjustments by discounting to take account of the fact that future dollars are worth less than present dollars). All of us are aware that the benefits of various types of programs may be considerably greater than the increase in earning power. The benefits may extend beyond the persons affected by the program and may extend beyond earning power and productivity. The absence of pain is, after all, a benefit, as is the increase in happiness. It is therefore the totality of benefits that we are interested in; the monetary benefits are a proxy measure, though how adequate the proxy measure is remains, in many cases, a moot question. Similarly, of course, the costs of the program are not equal only to the budgeted dollars but includes private costs, foregone income, and so forth. As we are interested in total benefits and must make every attempt to measure even those things that are hard to quantify, so too with costs.

The rest is simple: the benefits are our numerator; the costs the denominator. The result—the benefit-cost ratio—thus becomes a valuable guide to policy, for it is clear that the higher the ratio the "greater" the payoff. A program with a ratio of 8 is, therefore, to be preferred to a program with a ratio of 3 (and one cannot help but feel a twinge of sympathy for the program with a ratio of less than one—a program where a dollar spent does not yield a dollar in return).

There are many conceptual and statistical difficulties involved in computing the benefit-cost ratio. In my view these problems should not be glossed over by those who calculate these ratios. Because they are important and because they can affect the results significantly, any benefit-cost ratio must be carefully examined to determine its reliability. Nor can it be used in a mechanical manner in determining public policy. It provides us with information that may be helpful in making decisions, but it does not provide us with the decision itself. It adds to our information, but it is important to know its real meaning and significance and not merely to accept it. Indeed, in some cases it may add misleading information and we must, therefore, be careful to guard against simply accepting the results. I shall not at this time discuss the various conceptual and statistical difficulties involved in computing the benefit-cost ratio. Suffice it to say, however, that one of the overriding considerations is the fact that though benefits are broadly defined they are often narrowly measured. In the case of health programs, I am not at all convinced that the measure that utilizes future income as a determinant is sufficient. Utilizing future income can lead to bias in the type of health programs mounted and in the choice of population group that it will serve. It could bias us against programs for the poor (or prospective poor), against programs for infants whose earning days are still far off, against programs that alleviate pain or worry. I do not, you will note, say that such programs *will* fail to qualify for support. I say only that there *might* be a bias against them.

I think you will agree that all of this is relevant to the question of determining needs. For if we agree about the importance of public policy and the intimate relation between public policy and the projection of needs into the future, then I think it follows that our definition of needs must carry with it some implications for policy actions by the decision makers. It may be well and good to discuss absolute health needs as measured by physicians and by statisticians, but for the guidance of public policy these needs must be translated into measures of the benefits that meeting them would bring. It should be noted, of course, that there is little agreement even among physicians and biostatisticians on what future health needs or indeed even present ones really are. This means that public policy is even more likely to be guided by decisions concerning relative needs rather than absolute ones. It is for this reason that I feel it imperative that we all understand the perspective from which many are trying to guide public decision making.

But if needs are relative, how then can we project them into the future? It seems to me that we approach the problem by first recognizing the difference between the possible meanings attached to the words "needs," "could use," or "would like," "demand." The first is sometimes considered a medical judgment though, as I have indicated, it really extends well beyond that. The "would like" definition may include something considerably greater than "needs," at least if we recognize that much medical care may be considered a "luxury." It would be difficult, however, to establish any quantitative estimate of the "luxury" component. In a society that has conflicting needs and scarce resources individuals (and even governmental units) may allocate more to the health area than can be justified on the basis of investment policy or needs. I should think that even in our society many consumers of medical care purchase more medical care than is in some medical sense required (at least from the standpoint of physical health). The word "demand" has a special meaning for the economist, a meaning that is intimately related to prices and attempts to assess the quantity of medical services that would be purchased at various price levels for medical care (all other things remaining equal). In terms of available data, it is "demand" that can be most readily measured. We do have data on the utilization of medical services by various population groups with varying characteristics (e.g., age, sex, race, education, income, location). These statistics, which in general may be considered an expression of what the various consumer groups are willing to purchase at existing prices and at their present levels of income, do enable us to conduct some analyses of demand and to make some projections of future demand. Let me make clear that such analyses do not answer all the questions that we might have and in particular do not indicate in convincing fashion the impact that income constraints on availability of services have on the utilization of medical services by some parts of the population. Such analyses do, however, provide a starting point for analysis. Let me therefore discuss with you some projections that I have made for the next decade. These are projections of demand. They represent, I believe, minimal projections. I expect that demand would grow somewhat more than these projections indicate. It should be clear that they do not represent projections of needs. For parts of the population I have assumed utilization rates below what many of us would agree are needed or are equitable. Other parts of the population, however, may be receiving—and under my projections would continue to receive—more medical care than would be required. How these two forces balance is not clear, but I suggest that in a free market economy we may not have the choice of redistribution from those who are buying as much as they would like to have (and more than they need) to those who cannot buy what they would need or would like to have. In our economy we may have to allocate more to health than would be necessary in an aggregate sense because we are limited in our redistribution and reallocation policies. Thus we may be forced to a situation where some would have more than they would need but would have as much as they could buy while others would be raised above their present levels of consumption. We may have to guarantee minimum levels for all, even as we permit some to purchase care above that minimum level.

In my analysis I have assumed that in the future individuals with certain characteristics would behave as individuals with those same characteristics behave today. What I have done is to assess carefully the characteristics that many persons are likely to have in the future. Tomorrow's populations—more correctly the population a decade hence—will be larger than today's, will be better educated, will be slightly more urban, will have a somewhat higher proportion of Negroes, and will have higher levels of incomes. All of these characteristics impinge on the quantity of medical care that people purchase in today's market. They are likely to impinge on the quantity that people will purchase a decade hence.

The largest single influence will be the growth in population. The medical care delivery system must adjust to a population growth of approximately 14 per cent. Other demographic characteristics will not have significant impacts. The age-sex distribution of the population will change but—as in the case of other demographic characteristics—these changes are not very rapid in a period as short as a decade. Thus the impact on the medical care scene will not be great. I expect that age-sex changes would lead to an increase of approximately 1 per cent in the demand for physicians' visits. The impact of changes in the location of the population, both in terms of region and place of residence (urban or rural) and the change in the racial distribution of the population would add less than another 1 per cent to the demand for physicians' visits. The impact, however, of rising levels of income will be considable. Obviously the assessment of the possible influence of changes in levels of education and income (the two are intimately related) first requires that we estimate the increase in income that is likely to occur over the next decade. This is a difficult task to undertake, for income characteristics are much more volatile than are the demographic characteristics to which I have already referred. If we assume that the percentage growth in income in the next decade would be about the same as over the last decade (the income that I speak of is measured in real terms, that is, income growth net of inflation) we are, it seems to me, making a reasonable assumption. Were such an income growth to take place, the impact on the demand for physicians' visits would be about 7 to 8 per cent. I should point out that in this projection of the impact of income changes, of income elasticity, I am being somewhat conservative. I am assuming that a 100 per cent increase in income would lead, roughly speaking, to a 16 per cent increase in demand, but other economists have calculated significantly higher income elasticities. We need not at this time discuss the reasons for these differences. Suffice it to say that I believe my methodology, while perhaps a bit conservative, is correct and that the other estimates are far too high. The final source of increase in the demand for the services of the physician arises from the growth in demand as a result of Medicare and Medicaid. I assume that Medicare would increase the demand for physicians' services by, at most, 2 per cent. Obviously the increase in the demand on the part of the aged would be significantly greater, but since the aged are a small percentage of the total population, the impact on the aggregate demand is only on the order of 2 per cent. The impact of Medicaid is more difficult to assess, in no small measure because Medicaid will differ among the various states. But much of the increase that would result from Medicaid has already been incorporated into my estimate of the increase in demand due to rising levels of income. To put it in another way: if income rises rapidly enough, then many persons would exercise their demand for medical services through the market place into which they could now buy rather than through coverage under Medicaid. Thus if income grows less than I have assumed, Medicaid grows more; if income grows more, Medicaid grows less. But the total impact on aggregate demand of income growth plus Medicaid is not likely to be different.

Now if we agree that this estimate of the impact of change in the next decade on the demand for medical services—an estimate that yields almost a 25 per cent increase in the demand for services—is minimal, and if we agree further that even with income growth a considerable part of the population will still be at levels of income that are too low to enable them to purchase the medical care that they need or that would provide a more equitable distribution, it follows that we have a problem. If, further, we note how inequitable our present distribution of medical services is, how poorly served the poor are, how this inequitable distribution leads to social tension, then we may want to make even more medical services available to those who are moving out of poverty and surely to the poor themselves than they would be able to buy in the market place. All of this says that in terms of social policy the 25 per cent increase in demand is not likely to meet medical care needs as defined in terms of physical needs or alternatively in terms of social equity.

I could discuss the projection of demand and the differences between the projection of demand and the projection of need in greater detail. But I propose now to move on to some additional points. Let me first note that if the growth in demand exceeds the potential growth in the supply, we are likely to witness rapid increases in the prices of medical services. Such price increases will serve as rationing devices. This will have unfortunate consequences of course, for this particular form of rationing does not meet our social objectives in this area.

I think that it is fair to say that for some period of time, at least until the recent past, many have felt that medical care needs could be met by providing new financing mechanisms that would enable people who do not now have sufficient income to purchase the medical care that ognized that there were tremendous unmet needs on the part of parts of the population but they also thought that all that stood between those groups and the exercise of effective demand was a shortage of dollars. Today we feel differently about this matter. It is necessary to recognize that persons need dollars with which to purchase goods and services. I think that it is also necessary to recognize that unless the total supply of services is increased, and increased significantly, *and in particular ways*, the dollars that we would give to some to purchase care are not likely to compete effectively against the dollars that others have. All dollars are equal but some are more equal than others. To put it simply, I do not think that we are likely to find that the dollars or vouchers, or rights, given the poor, will cause medical care to be redistributed from the rich in their direction. I doubt therefore that the medical care needs of the poor—and they are many—will be met by simple actions, or even complex ones, on the demand side alone. The location of facilities, the location of physicians, the types of problems involved—all of these suggest to me that the poor would not be able to compete effectively for the scarce resources involved.

And this, therefore, leads me to my final point. What can be done to meet the medical care needs? I am now interpreting the title of the talk, "Medical Care Needs in the Coming Decade," a little differently. I am saying "What do we need to do about medical care in the coming decade?" Many of you are better equipped than I to fill in the details of the actions that would be required on the supply side. But I do think that there are some fundamental requirements that can serve as guidelines by which to judge specific actions:

1) I believe that we should recognize that there are certain population groups that by virtue of their income, race, or location are not well served by the existing medical care system. In the absence of changes in the medical care system, massive increases in supply would be required before the level of services available to these population groups would increase significantly. I believe that this costly method, which relies on some spillovers for these population groups, if you will, on a trickle-down theory, is to be rejected. It is costly and society has other needs as well. It may lead to overdoctoring for parts of the population even before the spillover effects are noticed. It assumes that the existing organizational structure of medicine would continue, and there is no reason to operate with that constraint.

2) If, therefore, we are to operate on the supply side as well as on the demand side, we should operate in such a way as to take on responsibility for the provision of services. To me this means that we must extend the changes in organization and in delivery systems, and must assume responsibility for the implementation of changes. We must give up some basic assumptions: that the physician should be a small entrepreneur in addition to being a physician; that American medicine is to be supported with government aid, but that every facet of it should be free to decide for itself where it will serve, whom it will serve, and how it will perform; that government can intervene to increase supply in the aggregate and to increase demand in the aggregate, but should not intervene in matters of organization and distribution of the available supply. Once these assumptions are given up it becomes necessary to organize systems of care that serve a population.

3) All of this is based on one fundamental requirement: that someone, somewhere, state that a particular population group is his responsibility. It seems to me that this is what is so conspicuously lacking on the American medical care scene. The military administration has said that the health and medical care of the group that serve in the military forces is its responsibility and it has organized a system to fulfill that responsibility. But who has said that Harlem is his responsibility? Who has said that Appalachia is his responsibility? It is true that *some* groups have said that *some* part of the medical needs of *some* part of the population are their responsibility. Thus we have had intervention by some medical schools, by some state departments of health, by some city departments of health, by the Public Health Service. But what is required, I think, is a change in philosophy or the strengthening of some emerging philosophies. What is required is that someone, somewhere, should say effectively: "This is my responsibility." Once that is said, that person or that group or that governmental unit will necessarily

be faced with problems of organization to meet that responsibility. Then, it seems to me, we are likely to move forward.

This, then, is what persons concerned with medical care need to do in the next decade in order to meet the medical needs of the next decade.

Social Control of Science and Technology

Michael S. Baram

Science and technology increasingly work changes in the complex matrix of society. These changes pervade our ecological systems and our physical and psychic health. Less perceptibly, they pervade our culture, values, and value-based institutions, such as the law. In turn, our values and institutions shape the progress and utilization of science and technology.

Science and technology have provided society with enormous material benefits and a higher standard of living and health. Yet these benefits have been accompanied by alarming rates of resource consumption and new hazards to ecological systems and health. Social response to these unexpected problems has been of a remedial nature—that is, how to diminish pollution through regulation and technology. However, since our values and institutions shape the progress and use of science and technology, the fundamental social response must come from change in these values and institutions. To the extent possible, this change should yield preventive or a priori controls.

This important task can be described as the need to formulate coherent and humane social controls on science and technology.

Of course science and technology are not discrete activities: They describe a process that ranges from basic research through applied research and development technology to application and use technologies. Most social change occurs during the latter stages, in which technology is manifested either in specific acts, such as organ transplantation techniques, or as part of a major public or private system, such as nuclear energy or computer applications.

Events throughout this process have become highly dependent on federal funds since World War II. In 1969, approximately 65 percent of the funds spent in the United States on basic and applied research and development technology were provided by federal agencies. This reliance on federal support provides even further justification for public interest in the social control of science and technology.

The most substantial expenditures and investments occur during the development technology stage, after a number of important decisions have been made about prototypes, production, application, and use. Of the approximately $17 billion of federal support for research and development in 1969, it is estimated that $5 billion went for research and $12 billion for development. Production and application activities undoubtedly involved billions more. Similar ratios prevail in the private sector.

These investments must be considered in human as well as economic terms, for it is during the development and subsequent stages that large numbers of engineers, administrators, managers, production and shop personnel, salesmen, and subcontractors commit their careers, personal values, and families—and ultimately their communities—to the specific technological activity or system. Therefore, all subsequent social controls must consider the political, economic, and human factors that have been developed.

Numerous social controls on institutions and individuals generating or utilizing science and technology have been developed over the years. Table 1 suggests, in general terms, what these controls are and where they function in the various stages of science and technology. The legal doctrines in the table all operate during the advanced technology stages—after decisions committing economic and human resources have been made and, normally, after injury has occurred. By this time, fully developed systems and practices are in use, without coherent controls.

This has led to condemnation of law as a modern system of control. As Jacques Ellul has said (*1*):

> The judicial regime is simply not adapted to technical civilization, and this is one of the causes of its inefficiency and of the ever greater contempt felt toward it.
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> Law is conceived as a function of a traditional society. It has not registered the essential transformation of the times. Its content is exactly what it was three centuries ago. It has experienced only a few fragmentary transformations (such as the corporation)—no other attempts at modernization have been made. Nor have form and methods varied any more than content. Judicial technique has been little affected by the techniques that surround us today; had it been, it might have gained much in speed and flexibility.
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> Faced with this importance of the law, society passes to the opposite extreme and burdens administration with everything that is the product of the times in the judicial sphere. Administration, because it is better adapted from the technical point of view, continually enlarges its sphere at the expense of the judicial, which remains centered on vanishing problems such as codicils, community reversions, and the like. These last, and all similar problems that are the exclusive concern of our law, are problems that relate to an individualistic society of private property, political stability, and judicial subtlety.

In specific terms, the legal system has not been responsive to new social conditions. For example, it has not functioned as an effective control on science and technology because it does not operate early enough in the process. Harold Green, in discussing this issue, has said (*2*):

> The basic question is whether our legal system is capable of imposing effective social control over new technologies before they inflict very substantial, or even irreparable injury upon society. It seems clear that we cannot rely on the courts alone to protect society against fast-moving technological developments. Judge-made rules of law always come after, and usually long after, the potential for injury has been demonstrated. . . .

This characteristic of retroactivity limits the ability of the legal system to respond to a number of modern social problems, in particular the harmful effects of science and technology and the problem of environmental deterioration. Retroactivity is inherent in a legal system based on the values and conflicts of the private sector of society. The courts have not been designed to serve as oracles or social planners, but to

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Table 1. Where social controls occur in science and technology.

Sources of control	Basic science	Applied science	Development technology	Production, application, and use technology
Scientific peer groups	×	×		
Professional associations				×
Federal government				×
Executive action		×	×	×
Agency programs	×	×	×	×
Agency regulation	×	×	×	×
Agency security classification	×	×	×	×
Congressional hearings		×	×	×
Congressional legislation and funding	×	×	×	×
Industry-consumer markets			×	×
Industrial associations and labor unions			×	×
Insurance				×
Crusaders and citizens' groups			×	×
Law				
Patents, copyrights, trade secrets			×	×
Torts				×
Constitutional rights				×
Land use			×	×
Consumer protection				×
Experimentation		×	×	
Education-ethics	×	×	×	×

grapple with actual conflict manifested in specific acts or injuries. They lack the technical, astrological, or other expertise needed for the diffiult task of evaluating the present, diffuse effects or the future effects of science and technology. Consequently, the courts are reluctant to impose controls and have rarely intruded on the substantive aspects of decisions of public agencies, which presumably are technically expert.

Judicial procedures that have reinforced concepts of justice and due process, such as statutes of limitations and rules of evidence and standing, have also brought an immobility to the law to the extent that it cannot respond easily to such issues as deleterious damage or public health.

Recent developments in environmental litigation have ameliorated some of these procedural obstacles, particularly the issue of standing for citizens' groups alleging other than economic injuries. But some feel that this brief honeymoon is already over. In Sierra Club *v.* Hickel (*3*), the Ninth Circuit Court of Appeals denied that the Sierra Club had standing, since it had not alleged that its members would be affected, beyond displeasure, by the scheduled action of the Department of the Interior. (This action was the approval of a commercial and recreational development, in the heart of Yosemite National Park, to be carried out by the Walt Disney Corporation.) This may indicate that the bounds of procedural flexibility have been reached.

The list of problems is incomplete, but sufficient to justify the conclusion of a recent law review note: "The passive nature of the courts and the difficulties encountered in their use make it clear that they cannot serve as society's primary instrument for technology assessment" (*4*).

To return to Table 1, the controls of the private sector are similarly clustered in the advanced technology and use stages. For obvious reasons, industrial decisions and insurance controls are implemented without full consideration for the public interest. Decisions are made on market or profit considerations, based on what the consumer wants or can be manipulated to want, and do not consider larger public interests in the preservation of natural resources or public health, for example. Advertisements boost the sales of items that are attractive to individual consumers, but that collectively erode environmental quality, other public interests, and, ultimately, private interests. Sales of snowmobiles to the new breed of armchair sportsmen have climbed to 500,000 annually and provide a noisome case in point.

The automobile represents the ultimate absurdity. The automobile birthrate is now treble the human birthrate in the United States: 10 million automobiles are produced for every 3 million human beings. Death rates occur in a similar ratio. Automobiles produce most of our air pollution, are dangerously designed, and are not economically recycled. How much longer can these absurd ratios and harmful effects be tolerated, despite the importance of the industry to the economy? Obviously, many of our problems labeled technology-induced or environmental are, in reality, the behavioral problems of a materialistic society. As such, we cannot expect effective private sector controls to emerge, nor can we expect courts to alter such "normal" behavior.

Crusaders and citizens' groups have recently proven somewhat effective as technology-curbers, but they have not provided coherent, a priori controls. Crusaders are in short supply, and citizens' groups lack funds, technical expertise, and national political strength. They can only attack discrete problems, often on a local scale, and must ultimately resort to the legal system with its shortcomings. Their task is made extremely difficult by the fact that, once again, substantial economic and human commitments have already been made in support of harmful developments, on a scale far larger than the immediate interests represented by such groups. Without substantial evidence of harm to public health, such groups appear to represent merely their own esthetic or otherwise elitist values, or a Luddite revival. This is not said to disparage such activities: They have served to educate and involve citizens, and they represent an exciting and valuable development.

The public agencies have actual and potential social control functions that cover the complete spectrum of scientific and technological activities. But this role is inextricably wound up with their several other functions, which include the promotion of certain activities for national purposes such as defense or the balance of payments. Reasons for their failure to exert social control have often been cited and are true to varying degrees: bureaucracy and inertia, ignorance and lack of sensitivity to noneconomic interests, fragmentation of authority by congressional design or by subsequent developments.

Legislation has proven to be no guarantee of implementation. The Refuse Act section of the 1899 Rivers and Harbors Act is a potentially powerful source of authority for combating most forms of water pollution as they occur.

Yet for 70 years it had been ignored by the Corps of Engineers and the Department of Justice.

The idea of reorganizing the federal agencies or creating new administrative bodies to better control science and technology has been under discussion for some time. Under this rational measure, one or several new and prescient groups would function as long-range planners with coherent control authority. For example, a single agency could, perhaps, determine national and regional energy needs and then plan, license for construction, and regulate in the public interest more effectively than the present multiple-agency condition. Reliance on teams of technical experts and experts from such other fields as law, health, and economics could be built into reorganization plans of this kind.

These are certainly steps in the right direction. Of our present array of social controls, perhaps the public agencies, which support most research and development, could effectively perform assessment and control functions when they are most important—before large-scale development and the commitment of economic and human resources.

Hugh Folk, in considering present and future social control by public agencies, has already discerned some pragmatic problems (*5*). Experts will once again be drawn from the same pool. Many of them will actually have contributed, in industry or government, to the problems they will be called upon to solve. Few experts will be able to apply their disciplinary background to a wider range of social issues. And experts will need extraordinary courage to function in a truly critical sense, since their careers will still be rooted in the same industrial-governmental-university milieu. What will happen to the expert who tries to serve the public interest by calling for a halt to a particular line of research? A test case is now before us involving radiation safety standards. John Goffman and Arthur Tamplin have challenged the Atomic Energy Commission and its affiliates in industry and the universities.

Folk's central thesis must be repeated here: assessment and control are essentially policy-making processes and, as such, will be embroiled in political controversy. He fears the repetition of the nonrational policy-making that occurs in our present agency framework and that results in agency establishment of "standards at levels slightly below that at which people complain vigorously . . . thus keep[ing] the public sullen but not mutinous." Designs for central assessment and control authorities must meet these issues squarely if real change is to occur.

Finally, let us briefly consider peer groups, well positioned to assess and control early in the basic and applied science stages.

Based on personal observation, in part, I do not think scientific peer groups presently have the objectivity or capability to function as coherent and humane social controls. The members of a peer group share the narrow confines of their discipline, and individual success is measured by the degree to which one plunges more deeply into and more narrowly draws the bounds of his research. There are no peer group rewards for activities or perceptions that extend beyond the discipline or relate it to social problems. Members are therefore neither motivated nor trained to relate their peer group activity to broader social concerns. Probably because of their closeness and commitment to their work, they are unable to objectively assess implications and recommend controls.

Genetic research today provides us with a case in point. It is proceeding rapidly in the United States and England, and, periodically, significant breakthroughs are announced. Members of the peer group and others have frequently discussed the potential applications of their work, and it has become a fashionable topic. Despite the potential for genetic engineering and its misuse for political and social goals repugnant to our professed values, this work continues at an urgent pace. I would think that the historical evidence of the political misuse of science and technology in this century would at least bring about a slight pause or slowdown in activities until our legal and other control systems had time to prepare principles regarding experimentation, as well as other public and private safeguards.

It is a disturbing experience to discuss these issues with biologists. Their responses avoid the central issue of slowing or suspending work to formulate controls and include the following:

► "If we don't do it, somebody else will";

► "Don't worry about secret and horrible developments—all work is done in large, expensive labs funded by the government";

► "Further work will improve the health of society and upgrade the gene pool";

► "Cloning of humans is at least 5 [or 10] years away";

► "Science is intrinsically valuable in its contribution to man's collective knowledge, and it must not be controlled for social purposes of any sort."

Self-enclosed peer groups cannot be entrusted with self-control, perhaps because of their narrow disciplinary backgrounds or self-interest, and perhaps because our educational system does not foster ethical and interdisciplinary values in professional training (*6*).

The social control of science and technology will be a troublesome and never wholly successful undertaking. It bears the potential to politicize and regiment intellectual activity, which has been realized in Russian genetics. Nor will the task lend itself to a specific solution—there are no administrative, legislative, or judicial panaceas.

Of course, it must also be recognized that future impact assessment and derivative control will always be limited, as man's intellectual and imaginative resources are limited. Even our measuring devices are still too crude to discern pernicious impacts in many cases. The earlier the assessment takes place in the process of science and technology, the more speculative it is. But the practice must begin, and develop, and pervade all the social control mechanisms we now have and may devise.

To begin, there are a number of reforms that can be introduced in our present array of social controls. Administrative agencies must be reorganized sensibly in light of new national objectives and available scientific and technological resources.

Legislation must be generated to provide guidelines for the administrative agencies similar to those provided by the National Environmental Policy Act (NEPA). Substantive and procedural duties are imposed by NEPA on all federal agencies to implement a broad policy of preventing and eliminating environmental damage. Section 102(2) of NEPA requires that the federal agencies, in their policies, recommendations, and other major federal actions affecting environmental quality, shall (*7*):

> A) utilize a systematic, interdisciplinary approach . . . in decision making which may have an impact on man's environment;
>
> B) . . . insure that presently unquantified environmental amenities and values . . . be given appropriate consideration in

> decision making along with economic and technical considerations;
>
> C) include in every major recommendation . . . and other major federal actions . . . [a] detailed statement . . . on (i) the environmental impact of the proposed action, (ii) any adverse environmental effects which cannot be avoided should the proposal be implemented, (iii) alternatives to the proposed action, (iv) the relationship between . . . short term uses . . . and long term productivity, (v) any irreversible and irretrievable commitments of resources which would be involved . . .
>
> D) study, develop and describe appropriate alternatives. . . .

We can only speculate about what impact NEPA will have on environmental quality. Perhaps its primary significance will be to instill certain *habits* and *values* in federal officials and the experts they consult: the habits of interdisciplinary assessment and consideration of alternatives, and a value system that would include health and ecological considerations.

NEPA will probably slow down the agency decision-making process, and this will help matters. Finally, NEPA will bring about the generation of information by federal agencies. This information should become available in useful form to concerned citizens who invoke the Freedom of Information Act (*8*). The agencies' broad-based studies of harmful effects and alternatives will be helpful, either because of contents or omissions, to environmental action groups. Hopefully, executive privilege and other exceptions to the Freedom of Information Act will not be invoked to the detriment of congressional purpose as expressed in NEPA. Unfortunately, this has already occurred in Soucie *v.* DuBridge (*9*), where the Office of Science and Technology report to the President on the SST was successfully withheld from conservationists.

Obviously, NEPA will also bring about some assessment and agency control of science and technology when environmental effects are predicted. However, there is a need for legislation, similarly grounded in a multiple-value system and the habit of assessment, that will more directly confront the need for a priori control of science and technology. This legislation should be directed at the substantial agency sponsorship of research and development, thereby regulating federal procurement and government contractor activities.

Independent adversaries must be fostered. A tax-exempt status ruling by the Internal Revenue Service would be a helpful first step for citizens' groups pursuing activities in the public interest—for example, groups that have demonstrated their concern for public health. Multiple-year grants to interdisciplinary groups, perhaps based at universities, could foster independent adversaries by establishing new career patterns. Congress, through its committee structure and reference service, should assist in this process.

Citizens should continue to press for responses from the legal system. Environmental litigation to date has been marked by ingenuity, but it lacks a coherent rationale. If Sierra Club *v.* Hickel is an omen of anything, it may be that the mere displeasure or the aggravation of elitist values of a citizens' group will not be sufficient to challenge agency and industrial actions that serve economic or public recreational interests, even though on a crass and commercial basis. Perhaps this is as it should be. Litigation to control environmental quality and science and technology should seek a coherent and important *raison d'être*—public health.

Public health—in both physical and psychic terms—includes esthetic and recreational values and the importance of ecosystems. It can therefore provide the nexus between citizen group social action or litigation and the public interest. The federal agencies, under NEPA, must now consider health effects. Establishing public health as the nexus does not simplify decision-making, but it can reduce subjective value clashes and will cause science and technology to be used in a self-evaluative and beneficial manner.

Finally, the most important social control must be discussed—education. The training and values of our professionals in law, engineering, and other fields must be responsive to the problems that beset society. The intense specialization that marks graduate education fosters narrow professionalism. Peer groups have not rewarded members who apply their training to problems that extend beyond disciplinary confines.

Our graduate schools and departments represent artificial divisions of knowledge and experience, and they deprive students of important opportunities and professional qualities. Substantive specialization and procedural barriers prevent students from working with colleagues in other disciplines and, often, from doing clinical work that is related to social issues. As a result, they are unfamiliar with the values, attitudes, and methods of other disciplines and unable to synthesize and apply them to social issues. These limitations in training are then reflected in careers and social problems.

No new degree programs will provide us with the answers. Rather, every degree program we now have must be enriched with interdisciplinary, clinical, and, preferably, problem-oriented components. Many exciting educational experiments, such as Cornell's "Science, Technology, and Society" program, are being conducted in institutions across the country.

Several innovative developments are taking place in the Boston area. At M.I.T., the school of engineering is moving to confront problems of biomedical engineering, public systems, and environmental quality. The civil engineering department has brought into its faculty and academic structure an interdisciplinary team made up of a political scientist, a lawyer, and an economist to work with the engineering faculty on water resources, transportation systems, systems engineering, and environmental quality. Engineering students can now enrich their academic programs with courses and research that relate their engineering disciplines to the full complexity of the social context in which they will eventually work. A number of engineering students have joined members of the Harvard and Boston University law schools' environmental law societies on projects confronting local and national environmental issues.

Professor Jerrold Zacharias is now working on adapting M.I.T.'s advanced degree programs to specifically train students for college teaching careers in science and engineering. The mastery of a discipline, educational methods and technology, ethical and legal materials, and interdisciplinary research are now considered to be important features of this development. Graduates will be expected to bring breadth and innovative qualities to their teaching careers and relate their discipline to the social context.

Finally, at Boston University Law School, the new Center for Law and the Health Sciences has established a program that enables law students to work with graduate students from other disciplines on health-related social problems. Student and faculty participants are drawn from different disciplines and institutions, and students receive academic credit through ad hoc institutional arrangements.

David Bazelon, chief judge of the Washington, D.C., Federal Court of Appeals, has played a major role in this undertaking, as chairman of the center. In a summer pilot program, 15 graduate students from Boston University, Brandeis, Harvard, and M.I.T. were divided into four interdisciplinary teams. Each team confronted a complex health problem: genetic counseling, health insurance reform, multiple-service health centers, and the training of mental health professionals. Each team contained a law student, medical student, economist, or urban planner and a student from a discipline particularly relevant to the problem—for example, bioengineering. Twelve faculty members, representing a number of disciplines, served as a general resource to the students at scheduled meetings and informal sessions.

Interdisciplinary education presents a number of organizational problems and a number of unique educational benefits. Much was learned from the summer pilot program, and the academic year program is now being implemented. Problem orientation has proven to be an important aspect of the interdisciplinary program, in that it forces students to learn, synthesize, and then apply their knowledge. At the same time, students are able to exercise considerable initiative in defining and working on problems in a context of competing values. The center hopes thereby to enrich the graduate education of a number of students and enable them to function effectively in health-related careers.

The social control of science and technology, through the training of new kinds of professionals, is one of the most important tasks at hand for law schools, schools of science and engineering, and other programs of higher education. This task must become an ongoing process, and it needs interdisciplinary cooperation and public support. Faculty in schools of professional training in medicine, law, and other fields, are needed to help build and implement these new programs of public service and must rejoin the university. In addition, these new programs must be related to the social system and values, for only through individual and collective wisdom and temperance, induced by an appreciation of the values of others, will we control science and technology in a coherent and humane fashion (*10*).

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6. See H. Morgenthau, "Modern Science and Political Power," *Columbia Law Rev.* **64**, 1386 (1964).
7. 42 U.S.C. 4331 *et seq.* A full review of the National Environment Policy Act is presented in an article by R. C. Peterson of Yale Law School (*Title I of the National Environmental Policy Act of 1969*). It is available from the Environmental Law Institute, 1346 Connecticut Ave., NW, Washington, D.C.
8. 5 U.S.C. 552.
9. Soucie *v.* DuBridge, U.S. District Court, District of Columbia (1970).
10. Further information on the programs discussed above is available from the author, Room 1-376, M.I.T., Cambridge, Mass. 02139.

WHITE HOUSE SUPERSTRUCTURE FOR SCIENCE

The all-pervasive influence of the evolving White House superstructure for science—men, organization, and policies—continues to have profound effects, both real and imagined, not only on American science but on American society as well

Next month's Presidential election looms large in implications for science and technology. Not the least of these is the effect of the outcome on the growing movement toward direct White House control of the federal science establishment—a movement which has characterized the Kennedy-Johnson Administration. Whether this movement will continue and at what pace will depend largely on the will of the man who is elected President next month.

This movement, principally in just the past three years, has provided a tight new superstructure for federal science and technology. Its influence is international as well as national in scope. It weaves through our entire social fabric—political, industrial, educational. For it is this handful of people who to a lesser or greater degree hold sway over the Administration's plans in science and technology—plans which this year will mean a federal outlay probably in excess of $15 billion.

Dubbed by its critics as the Executive Branch "innersanctum for science," the White House science staff consists of the Special Assistant to the President for Science and Technology and his 19 aides. The degree of actual power which this group possesses and the manner in which it exercises it are topics of much controversy and confusion.

The controversy and confusion is due in no small measure to the self-generated aura of mystery that has surrounded the group and the way it operates. Aside from that of the Special Assistant to the President for Science and Technology, even the identity of the science staff of the White House is known to relatively few of the nation's scientists and engineers. Yet, working quietly behind the scenes, the group represents perhaps the biggest single influence on national decisions affecting science and technology.

The group's power—as well as much of the confusion surrounding the White House science structure—stems from the multiple roles and responsibilities of the Special Assistant to the President for Science and Technology (known informally as the Presidential science adviser). He is, at the same time, personal science adviser to the President, chairman of both the President's Science Advisory Committee and the Federal Council for Science and Technology, and director of the Office of Science and Technology.

Despite his multiplicity of titles, the Special Assistant to the President for Science and Technology actually has just two responsibilities. They are to advise and assist the President on all matters of national policy affected by or pertaining to science and technology and to evaluate and coordinate the total federal program in science and technology. All the various jobs he holds in addition to that of Special Assistant are tied directly to these two responsibilities.

Thus reduced to fundamentals, any real power of the Special Assistant to the President for Science and Technology, and consequently of his staff, is predicted to a very large extent upon his access to, and influence on, the President in determining who gets how many of the federal dollars for science and technology.

Rapid Development

The evolution of this new superstructure for science in the Executive Branch has spanned just a short seven years from birth to full maturity. Prior to 1957, the only formal scientific body in the White House was a relatively obscure science advisory committee submerged in the old Office of Defense Mobilization (now the Office of Emergency Planning). In the period just prior to the launching of the first Sputnik, this committee—shortly to be vitalized into the powerful and prestigious President's Science Advisory Committee—was relegated to the role of advising the President through the director of ODM, not directly. It was concerned principally with scientific and technical aspects of defense mobilization and national security.

In the great scientific flap in this country following the Soviet's spectacular launching of the first man-made satellite, President Eisenhower hurriedly set about to equip the White House with some sort of scientific and technical competency. He created the top-level post of Presidential science adviser and assigned to its the primary task of taking stock of this country's scientific and technical resources and coming up with ways to best bolster and mobilize them to meet this new Soviet challenge.

To aid the Presidential science adviser in his job, President Eisenhower yanked the existing science advisory committee out of ODM and made it advisory to the President directly, thus greatly increasing its stature, powers, and, by the same token, its appeal to the scientific community. The Presidential science adviser was made chairman of the revitalized committee.

The President's Science Advisory Committee (PSAC) is composed of 18 of the nation's most distinguished scientists and engineers drawn from industry, the universities, and from other nongovernment areas. The President appoints its members for four-year terms. PSAC meets on an average of two days a month and is concerned with major issues bearing on this country's scientific and technological posture. It undertakes studies both on its own initiative and in response to specific requests from the President. The White House describes a primary characteristic of PSAC as "the role it plays in blending and integrating governmental and nongovernmental views to achieve a total approach to problems involving science and Government."

In 1959 the mission of the Office of the Special Assistant to the President for Science and Technology was elaborated further by an executive order which created the Federal Council for Science and Technology. The council is composed of the top policy-level representative from each of the federal agencies involved in science and technology, and the Special Assistant is chairman. It was designed to provide a coordinating mechanism for the total federal effort in science and technology. In the process it also served to tighten White House control over scientific programs and policies of the federal agencies.

Establishment Complete

The framework of this new White House superstructure for science was completed in 1962 by President Kennedy's Reorganization Act No. 2, which created the Office of Science and Technology (OST). The director of OST is charged generally with assisting the President in "coordinating federal science and technology functions." More specifically he is to "advise and assist" the President with respect to:

- "Major policies, plans, and programs of science and technology of the various agencies of the Federal Government, giving appropriate emphasis to the relationship of science and technology to national security and foreign policy, and measures for furthering science and technology in the nation."
- "Assessment of selected scientific and technical developments and programs in relation to their impact on national policies."
- "Review, integration, and coordination of major federal activities in science and technology, giving due consideration to the effects of such activities on nonfederal resources and institutions."
- "Assuring that good and close relations exist with the nation's scientific and engineering communities so as to further in every appropriate way their participation in strengthening science and technology in the United States and the Free World."
- "Such other matters consonant with law as may be assigned by the President to the office."

OST did not represent so much a new White House scientific function as it did a formalizing of already existing functions. It gives statutory permanence for continuing Presidential staff support to the Special Assistant to the President for Science and Technology, PSAC, and the Federal Council for Science and Technology. The creation of OST has more than tripled the number of permanent science staff members available to the President.

OST was the Kennedy Administration's answer to growing Congressional unrest over the lack of coordination and central control of the Federal Government's burgeoning research and development effort. Congress was also demanding a single authority it could call in to answer its questions on the Administration's plans and policies on matters dealing with federal science and technology.

Under its charter, the National Science Foundation had been charged with advising the President on coordination of federal research policies and evaluating the research programs of government agencies. But for a number of reasons, NSF had been unwilling or unable to exercise this authority, mainly because it is on the same organizational level as other agencies. Its management felt that the agencies would balk at having NSF, in essence a sister agency, ride herd on their R&D programs.

So the Administration felt it needed to create a new office at a higher level than the agencies themselves to review and evaluate the total federal R&D effort and to meet demands in Congress for a better source of information.

The kind of information Congress wanted had not been fully available to it in the past. Presidential science advisers, for example, on numerous occasions appeared before Congress to describe in a general way various Administration plans and programs. But they have refused to answer certain more specific Congressional inquiries on the grounds that to do so would violate the doctrine of executive privilege. This doctrine says in effect that communications between the President and his advisers shall remain confidential.

The creation of this new White House office, according to Administration spokesmen at the time OST was proposed, would reduce these pressures on the Presidential science adviser. Congress could instead call in the director of OST to testify for the Administration on matters dealing with science and technology. In this way, they explained, the confidential relationship that exists between the President and his science adviser would not be impaired.

This would seem to be an adequate solution to the problem were it not for the fact that, ever since the creation of OST, its director and the Presidential science adviser have been one and the same person.

Kennedy-Wiesner

During the Kennedy Administration, with Dr. Jerome Wiesner as spearhead, White House control of federal science and technology reached a new high. Dr. Philip H. Abelson, editor of *Science* and probably the most vocal critic of the White House's power structure for science, remarked at the height of this regime shortly before Mr. Kennedy's assassination, "Dr. Wiesner has accumulated and exercised more visible and invisible power than any scientist in the peacetime history of this country."

It was also during the reign of Dr. Wiesner that the Presidential science adviser emerged for the first time as a prominent and influential figure in affairs of state.

To a large measure, this increase in the job's power and prominence can be attributed to the close, informal relationship that existed between the Chief Executive and his science adviser, a closeness and degree of influence which the two previous Presidential science advisers did not enjoy with their leader, Mr. Eisenhower. To some degree it stemmed from the way the late President operated—the relatively free hand Mr. Kennedy gave his top advisers as a show of his confidence in their abilities.

Another contributing factor to the new and larger scientific role of the White House under Dr. Wiesner—one that is sometimes overlooked by critics of the White House science movement—is that about that time the Government really began to broaden the scope of its involvement in science and technology. The two previous holders of the office under President Eisenhower, Dr. James R. Killian and Dr. George Kistiakowsky, had been largely preoccupied with the defense and space efforts. But as the Government began to move more heavily into support of other scientific and technical endeavors—oceanography, environmental health, consumer protection, and the like—the role of the Presidential science adviser correspondingly increased both in scope and in importance.

But a good deal of the credit (or blame, as you choose) for the office's new power went to Dr. Wiesner himself. Brilliant, ambitious, and tough, the MIT electronics whiz was the embodiment of the Kennedy kind of New Frontiersman.

Dr. Wiesner quickly adapted to the milieu of Washington and proved to be an eager and fairly adept performer in his own right in the political arena.

Forceful and confident yet with an easy, outgoing manner, Dr. Wiesner was regarded generally by both friend and foe alike as an articulate and effective salesman for Administration views relating to science and technology.

But trouble was brewing beneath the surface. Some scientists and others in Government complained privately about what they called Dr. Wiesner's "take charge" manner and "high-handed" methods. From agencies with scientific and technical programs came scattered rumblings of undue meddling in their affairs by Dr. Wiesner and his staff. The cry was taken up by a small but vocal group of scientists from the private sector. These critics accused Dr. Wiesner of attempting to mastermind and control the entire federal effort in science and technology. They labeled him the self-ordained "czar of American science."

This smoldering controversy flared into the open briefly, then died suddenly with Dr. Wiesner's decision last fall to return to academic life. The tragedy that followed shortly thereafter in Dallas put an end to the issue at least for a time.

Winds of Change

In January of this year, Dr. Donald F. Hornig, a soft-spoken, 44-year-old physical chemist, took over as Special Assistant to the President for Science and Technology. Named as Dr. Wiesner's replacement by President Kennedy shortly before his death, Dr. Hornig assumed office at a time of change: change in the White House precipitated by the Kennedy tragedy and change in the political climate of the nation toward science and technology generally. Both Dr. Hornig and his office have been caught up in those changes.

Under President Johnson and with Dr. Hornig, the job of Presidential science adviser at first seemed to have taken a reverse turn to the more muted days of President Eisenhower. That is, it seemed to be more one of a behind-the-scenes adviser and less one of a direct participant in affairs of state. As one veteran Washington scientific observer remarked shortly after Dr. Hornig took office, "For better or for worse, the freewheeling days of the Kennedy-Wiesner regime—a period of unprecedented scientific influence in this country's affairs—have come to an end."

In truth, however, the apparent change did not represent a decrease in the powers or influence of the Presidential Science Adviser. It really reflected a change in the methods of operation both of the White House itself under President Johnson and of the new science adviser.

President Johnson brought to the White House his own methods of doing business which differ significantly from those of his predecessor. The casual, almost family-like relationship that existed between President Kennedy and his top White House aides was replaced to a large degree by a more formal businesslike working environment.

This change in the internal workings of the White House had a direct bearing on the powers of the Presidential science adviser. Dr. Hornig still does not enjoy the close personal relationship with President Johnson that his predecessor, Dr. Wiesner, did with the late President. Prior to taking office, he had only a nodding acquaintance with President Johnson. And unlike Dr. Wiesner with President Kennedy, Dr. Hornig is not a member of President Johnson's select inner circle of confidants. While he thus may not make his voice heard on broad topics to the extent Dr. Wiesner did, when it comes to scientific and technological matters, the President turns to him first for advice. Thus, there has been no weakening of the Presidential science adviser's role in guiding the nation's over-all plans for science.

The changing role of the Presidential science adviser from the overt and public display of power by Dr. Wiesner to the behind-the-scenes "persuasion" of Dr. Hornig is due in no small measure to the sharp differences in personal makeup between Dr. Hornig and his predecessor. A deliberate, somewhat reticent man, Dr. Hornig so far has succeeded in avoiding the hard glare of publicity which focused attention so often on the outspoken and often volatile Dr. Wiesner.

Dr. Hornig continues to align himself solely with the scientific community and has been well received generally by scientists both inside and outside the federal establishment. He is a scientist who has involved himself in Washington with only broad-scope scientific issues. He had judiciously avoided being drawn into the political arena—a jousting area Dr. Wiesner neither succeeded in skirting nor apparently attempted to skirt.

There is every indication that regardless of who is elected President next month the White House science organization will continue its dominant role. The level of federal spending for space and defense R&D appears to be reaching a plateau. The Democratic Administration now has turned more attention to long-standing problems of society—poverty, disease, environmental pollution, consumer protection, and the like—all of which have large scientific and technical components. Thus, its policy of increased federal involvement in science and technology, coupled with the building pressures from an ever-mounting federal budget, should demand firm White House control in all areas.

Sen. Goldwater, on the other hand, deplores "big government." If elected President, he may work for a reduction in the Federal Government's participation in many areas in which he feels it has no business being involved. His philosophy of less, rather than more government, plus his call for more fiscal responsibility in Government, would seem to favor an over-all reduction in federal spending for science and technology—with the one notable exception of that related to defense—and, correspondingly, a tighter White House grip on scientific expenditures.

One thing is certain, however. Should Sen. Goldwater become President, there would be at least a few personnel changes in the White House science staff. The Special Assistant to the President for Science and Technology and probably his top aides would be replaced by people who more closely share Sen. Goldwater's political views.

Dr. Hornig Appointed

A renowned scholar, scientist, and teacher, Dr. Hornig is a highly respected member of the scientific community who brought to the White House an intimate knowledge of the major scientific and technical issues that face this nation. The Harvard-educated (B.S. 1940, Ph.D. 1943), Milwaukee native came to the job from Princeton University where he is Donner Professor of Chemistry and chairman of the chemistry department.

During World War II he was research associate at the underwater explosives research laboratory at Woods Hole, Mass. Later he served as group leader in the Manhattan Project at Los Alamos Scientific Laboratory.

After the war, Dr. Hornig joined the faculty of Brown University. He was director of the Metcalf Research Laboratory there from 1949 to 1957 when he moved to Princeton.

His research interests have included molecular and crystal structure, infrared and Raman spectra, shock and detonation waves, relaxation phenomena, and fast chemical reactions at high temperature. He has published about 70 scientific papers in these areas.

When the magazine ***International Science and Technology*** criticized the appointment of Dr. Hornig, labeling him "a virtual stranger to the Washington scene," an irate Chris Hornig, age 10, quickly rose to his father's defense. "My father has served for three Presidents," he informed the monthly's editor, "and is in Washington so much that by now he is a virtual stranger to me."

Dr. Hornig has been a member of the advisory panel for chemistry of the National Science Foundation, and from 1956 to 1961 served as a member of the physics advisory committee, Air Force Office of Scientific Research. In 1959 he was appointed to the Space Science Board of the National Academy of Sciences.

He was named to the President's Science Advisory Committee in 1960 by President Eisenhower and was later reappointed by President Kennedy. He was an adviser to the late President during the 1960 Presidential campaign and later served on the Kennedy Task Force on Space to help formulate policy in this field for the new Administration. In 1962-63, Dr. Hornig was a member of the delegation headed by National Aeronautics and Space Administration Deputy Director Dr. Hugh Dryden which negotiated the agreement with the U.S.S.R. for cooperation in certain space activities.

Dr. Hornig is a member of the American Chemical Society. He is also a Fellow of the American Physical Society, of the American Academy of Arts and Sciences, and of the Faraday Society, London. In 1957 he was elected to the National Academy of Sciences.

Science Staff

The controversial Office of Science and Technology, which comprises the permanent science staff for the White House, is a generally loose-jointed organization, although more clearly defined areas of individual responsibilities are taking shape. For practical considerations, it is a two-tiered organization with the director, Dr. Hornig, and his deputy on one level and an 18-member professional staff strung out along a horizontal plane on the other. In a sense, these 18 people are junior staff officers to the President in their individual areas of responsibility and carry considerable weight in scientific and technical policy considerations. The entire operation is housed in the antiquated Executive Office Building near the White House on Pennsylvania Avenue.

The scope of individual responsibilities of the OST staff men is enormous. For example, some 28 federal agencies are involved in one way or another with water resources alone. Thus, the OST staff man charged with responsibilities for water resources must somehow keep track of all these goings on plus activities of state and local governments as well as industry—not to mention those of Congress.

The size and complexity of its job—coupled with Dr. Hornig's desire to keep the office a small, flexible, and highly mobile operation—forces OST to rely heavily on the use of consultants via the *ad hoc* committee approach and to borrow people with hard-to-find skills from within Government whenever they are needed and wherever they can be found (a fact OST tries to play down for fear some Congressmen may frown on this practice).

In practice, the *ad hoc* committee approach works like this: A problem is either identified within OST or PSAC, or perhaps referred to it by the President. A group of knowledgeable people is assembled, usually with a member of PSAC as chairman, to hear testimony, to analyze the problem, and to recommend actions to be taken. When the work of the panel is complete, usually within a few months to a year, the panel is dissolved.

OST has found from experience that specialists usually don't work out well as permanent staff members. In the first place, the jobs do not lend themselves to specialization. Individual responsibilities are usually too broad.

"It is difficult, if not impossible, to find a man who is a specialist in all the areas he may be called upon to cover," an OST official points out. "Even if you could find one, you probably couldn't get him. That kind of person is usually serving at the level of the President's Science Advisory Committee."

"The ivory tower scientist," he goes on, "finds himself in a different world in Washington. Many of them just can't seem to bridge the gap between the two. For the kind of work our staff has to do, we have found that it is better to have people who can understand both aspects of the job, the scientific and the political. They must be able to get at the heart of scientific problems and then recommend a course of action to chip away at them while keeping within the bounds of political reality. That is, they must chart some middle course that takes into account all those things it might be nice to do, the things that absolutely must be done, and the size of the program that Congress will stand still for."

Is the OST staff as big now as it is going to get? According to Dr. Hornig, this is difficult to say. "Much of the past success of the office has been due to the policy of focusing on relatively few big issues and collecting expertise as it was needed," he explains. "The *ad hoc* panel with staffing from the office has been the most effective operating mechanism, and I for one wouldn't like to see a new bureaucratic superstructure grow here in the White House."

But, he points out, the problems on which he is experiencing more and more pressure are those of coordinating the science activities of the Federal Government and developing integrated approaches to problems. These, he says, are thorny problems because they are not particularly amenable to solution by outside consultants.

What happens in this regard will depend largely on how effective the various coordinating mechanisms such as the Federal Council and its committees can be made. Otherwise, "to do the job properly will require a considerably larger staff than we now have," Dr. Hornig says.

Criticism

The Presidential science adviser and his staff have been portrayed by some members of high standing in the scientific community as an unscrupulous, power-hungry band of scientific incompetents bent on complete domination of federal science activities at all levels. To get its way, one is led to believe, this group might resort to such sinister devices as budgetary blackmail, rigged agendas, and stacked committees.

Much of this criticism was laid at Dr. Wiesner's door. And his departure seems to have cleared the air somewhat. But despite the changes in leadership, the White House science complex is the same now as it was under Dr. Wiesner. Dr. Hornig has taken it upon himself to defend it against these charges, which he brands "ridiculous."

A close examination of the facts would tend to bear out Dr. Hornig on many points. For example, some critics would lead one to believe that agency officials sit quaking by the phone awaiting the next pronouncement from the White House.

True, at the policy-making level Dr. Hornig and Company have considerable say about the over-all size and direction of an agency's R&D effort. But both from observation and private conversations with key agency personnel, C&EN can find little evidence that the White House science staff is pulling the strings for the day-to-day operation of the Federal science establishment.

On this point, Dr. Hornig perhaps best sets the record straight when he says, "There are competent, strong agencies administering scientific and technical programs. When our views differ from theirs, none of them simply give up their position and adopt ours.

"In many ways, our most important task is to provide alternative views to the President. Most things we deal with, incidentally, don't originate here. They would if this were a hierarchy as some people have branded the office.

"Our function tends to be more of a review body, encouraging some things and discouraging others which have been proposed elsewhere. The exceptions are those problems of very general significance that can't really be approached from any single agency because they straddle many—such things as the integration of the entire federal scientific effort, the determination of over-all scientific and engineering manpower needs, and the coordination of scientific and technical information."

Admittedly, there is scattered resentment of the White House science staff within the agencies. This is understandable when a new office is created at a higher level to ride herd on agencies which previously were free of any such reins. Old empires go crashing. Toes are stepped on. Feathers are ruffled.

Some agency people are still nursing grievances from the early days of Dr. Wiesner's reign. In his zeal to carry out his duties during a period of rapid expansion in the scope of White House responsibilities for science and technology, Dr. Wiesner was sometimes guilty of neglecting the social amenities and protocol in his dealings with the agencies.

Dr. Wiesner is described by a number of his close associates as being "completely intolerant of mediocrity," and he generally made no pretense at hiding this fact.

The most vocal critics of science operations at the White House level are a relatively small group of scientists who for the most part have dedicated themselves to scholarly achievement and not to the hard realities of trying to run a $15 billion-a-year R&D enterprise subject to tremendous political, economic, and social pressures in addition to scientific ones.

They charge, among other things, that national goals in science and technology are set arbitrarily by the White House without proper regard to whether they represent the thinking of the majority of the nation's scientists and engineers. They imply that the White House is not getting the best advice available on scientific and technical matters, that it is handpicking its advisers from a small select group of scientists who are for some unexplained reasons favorites of the Administration or of the Presidential science adviser. And they don't think much of the caliber of the White House science staff in general.

The big controversy in recent times about national scientific goals is that raised over the late President Kennedy's decision in 1961 to make an all-out effort to land a man on the moon by 1970. As in any general policy decision of this magnitude, considerably more than just purely scientific considerations were behind the President's decision, factors which the Chief Executive should have been in the best position to know about. And even a number of the moon project's biggest critics today concede that despite the project's scientific shortcomings, it has provided a much-needed shot in the arm for U.S. science and technology in general and in scientific and engineering education in particular.

The criticism of the competence of the supporting White House science staff seems to be based in large part on its alleged lack of scientific stature. The critics resent appointment to important positions related to science of men who have risen by standards other than their own.

While generally displeased with the way scientific decisions are arrived at in the White House, critics do not advance any alternative systems they feel would be better suited to the country's needs. One is left with a decided impression, however, that they would prefer a more "democratic," or broader sampling, process. Perhaps they would favor some sort of national poll to assure that everyone concerned has a say in decisions involving science and technology.

Opponents of the present White House structure for science and technology also are not consistent in their arguments. For example, in one breath they express fear for what could happen by placing so much power in the hands of one man. And in the next they accuse the man of being largely ineffectual, leading one to believe that he is not using the power that has been given him.

Power

How much actual power do the Presidential science adviser and his staff really have? Actually, the office has no intrinsic power at all. Dr. Hornig trys to clear up the power question in this way: "Any office connected with the White House does not have power as such. It has influence, which is not quite the same thing. This is to say that the commodity we deal with is advice, and advice can be taken or ignored. Our advisory role would imply strong power if there were not alternative sources of advice which, of course, there are."

Semantics aside, though, the office has the wherewithal to bring considerable pressure to bear in the determination of the size, scope, and direction of the federal effort in science and technology. It reviews all major federal programs and policies which have any scientific and technical content or implications and it convenes groups of experts to consider their merit. As chairman of the President's Science Advisory Committee and the man who is in the best over-all position to keep abreast of emerging scientific trouble spots, the Presidential science adviser plays a dominant role in selecting the subject matter for PSAC's bimonthly meeting.

The major source of the office's strength is its voice in money matters. Critics have charged the office with exercising nearly absolute control over agency budgets for science and technology. They maintain that the Director of OST has become, in effect, Director of the Budget where scientific matters are concerned.

Dr. Hornig calls these charges "highly exaggerated." The office does review the scientific and technical aspects of the budgets of all federal agencies. But, as Dr. Hornig points out, the thoroughness of his review is limited by the size of his staff. "It's not a detailed review of every entry," he explains. "We try to sort out what we call budgetary issues and subject them to varying degrees of analysis."

The situation is the same on budgetary matters as it is on general matters of advice, he claims. "We work closely with the Bureau of the Budget, true. And we present our views on programs; that is also quite correct. Our views are sometimes accepted; also true. But sometimes they're not."

Drawing up the federal budget, he explains, is not as simple or clear-cut a procedure as some people would make it out to be. "It isn't just an exercise of someone's proposing a program and someone else higher up accepting or vetoing it."

Here is Dr. Hornig's account of how the budget-making process works:

"An agency proposes a program. Say this office, for a number of reasons, doesn't like it—we don't think it is well advised. Or, and I think this point is often ignored, we think it ought to be much bigger. At any rate, the Bureau of the Budget takes an independent position: It knows our position and that of the agency, and it knows from its own budget analysis the over-all budgetary restraints.

"At this point in the scheme of things, there is a discussion of the reasons on all sides. BOB tends to look at things in the over-all budgetary context. We tend to look at them as to how they fit into the total scientific and technical picture. The agency is concerned with how to carry out its statutory mission.

"Usually these differences are resolved by talking them out, although from time to time we must take a strong position. But even if they cannot be resolved in conference, there is still no veto. If they are major issues, the President can make the decisions. Then one tries as carefully as possible to crystallize the issue and the alternative courses of action for him. But in relatively few cases where the issue is drawn is the decision made by the President."

In Action

The office functions both on its own and at the request of the President. The series of studies on scientific manpower conducted by the office, for instance, were at the request of the President. The office's current study of the National Institutes of Health's extramural research program also originated with the President. These are just two of many jobs which the office has undertaken at the request of the President.

The majority of the time the office is a self-starter. As Dr. Hornig sees the office's role, "On most scientific matters, if we do our job well, it's up to us to anticipate problems before the President."

What spurs the White House organization into action? According to Dr. Hornig, it is nothing more complicated than the recognition of a problem. He uses the recent pesticide incident as a typical example of how these things get started.

"We might as well face it squarely. Maybe we should have recognized the problem earlier, but Rachel Carson published a book that caused considerable public discussion and got members of the President's Science Advisory Committee to scratching their heads. Violent discussion followed in which a strong segment—in this case, many of the chemical people—said it was a scurrilous book and was not based on any sort of fact, and so on.

"There was another equally strong set of voices, possibly not entirely rational in all cases, which said the situation was even worse than she portrayed it. As a result of this, PSAC decided there certainly was a problem that ought to be looked at.

"That was step one, and certainly not uncharacteristic. We don't usually start from published books; in fact, it's the only case I know of. But in any event, the first step is always the recognition of a problem.

"The second step was also characteristic. We selected what we considered a highly competent panel for the purpose. It went into the problem, produced a study, made some recommendations. Some of the recommendations involved actions by federal agencies, and to a considerable extent they have been put into effect. Some of them involved legislation. Bills containing most of those recommendations have been introduced, and some of them have been enacted into law.

"Finally, some of those recommendations haven't yet been put into effect. We still do not have a completely unified policy on pesticides. This is partly a result of the fact that the problem comes up in agencies which are oriented entirely differently. The Department of Agriculture, for example, is interested in the production of crops and is interested in pesticides from that point of view. The Department of Health, Education, and Welfare is worried about the effects of pesticides on human health. Given these different points of view, it's a problem to get a really unified approach. But we're moving in that direction and have made a lot of progress."

Laurels

Dr. Hornig feels that the White House science staff has made major contributions in a number of different problem areas. A good many of these achievements, he points out, have been in highly classified areas. And naturally they cannot be talked about openly today, which makes it impossible for those on the outside to get a true picture of the group's accomplishments.

But among the major ones that can be talked about, Dr. Hornig rates high the steady progress that has been made by this group in bringing some coherence into the federal system of supporting research. "The office," he points out, "has not tried to pull things together here in the White House, which would be the case if one were 'building an empire.' Instead, it promoted the idea of having assistant secretaries of research and development in all of the major agencies concerned. This has resulted in lifting the scientific competence to the policy-making level and in bringing a new and greater awareness of science in the agencies."

Congress' approval this year of the National Science Foundation's proposal to establish a "centers of excellence" program marked the successful climax of a hard uphill fight by both Dr. Hornig and his predecessor to increase the number of top-notch scientific and engineering schools in the country and thus provide new sources of advanced-degree personnel. The program is budgeted for $25 million this fiscal year. Maximum grants to individual schools will probably be about $5 million.

The need to develop these centers of excellence stems from the fact that in the past the bulk of the Ph.D.'s in this country have been graduated from a small number of schools. These same schools have also provided the research leadership and attracted a major share of federal research funds. In fiscal 1962, for example, 10 universities accounted for nearly 40% of total funds.

Since 1940 the number of Ph.D.-producing schools has been expanding steadily, but as the need for more people with advanced training grows and as the have-not sections of the country look to universities as a means of invigorating their economies there has been increasing pressure to speed up the process.

Under the new program, NSF will make grants to institutions which, in its judgment, "have substantial potential for elevating the quality of their scientific activities."

Right now Dr. Hornig is trying to get federal agencies that support research in the universities to agree upon a plan to give the schools a freer hand in the way they can use this money.

He feels that the project system, by which grants are made to individual investigators or groups on the basis of proposals which are judged by juries of peers is a major strength of American science. However, the project system in practice tends to neglect support for instruments which are used generally rather than for a specific project or service facilities such as machine shops, computers, and libraries. It makes it hard to support beginning investigators who are not yet in a position to write convincing proposals.

To do these things he has proposed that a small part, perhaps 3%, of the money in each project be made available to local administrators so the pooled funds can be used in ways which would improve the performance of all projects and strengthen research groups as a whole.

Trouble Spots

Dr. Hornig sees a number of other pressing scientific trouble spots. Perhaps the most central one, he feels, is the need to establish a better rationale for the level and kind of federal support of fundamental science. This problem is brought into sharp focus by today's changing political climate toward science and technology generally. The Government's R&D programs and policies are being held up to the most searching analysis to date by an increasingly budget-conscious Congress alarmed at the snow-balling of federal spending for research and development. Legislators no longer view science as a scared cow. In short, the era of the blank check for R&D seems to have come to an end.

Up to now, the nation has based its support of fundamental science largely on national security arguments, except in the health field, Dr. Hornig points out. "If security demands should relax, we will have to face squarely the problems of a more general rationale for supporting science," he says. "I don't think this is a basic problem. It is just one that has to be faced.

"At this time more than ever before, the nation must weigh its scientific programs against their cost to acquire a better understanding of their importance in relation to national goals," he says. "In particular, we must have a clearer view of the role of basic research."

Dr. Hornig thinks that basic research sometimes has been oversold on a pie-in-the-sky basis. "The public and the Congress are entitled to a more reasoned analysis of just what is the role of basic research," he feels.

Another pressing problem Dr. Hornig points to is the need to learn about the relationship of R&D to economic growth. "We simply don't understand it in quantitative terms," he says. "We need to have the answers to such questions as: How do you stimulate the growth of the economy? What, if anything, should the Federal Government do along this line, and how should it do it?"

SCIENCE AND PUBLIC AFFAIRS

Scientific Advice for Congress

A veteran legislator suggests that current proposals are overlooking some realities of legislative life.

Clinton P. Anderson

One of the results of the growing federal involvement in science and technology has been a growing uneasiness in Congress about its own ability to oversee programs in these areas effectively. The number of inquiries into the general state of science-government relationships undertaken recently is a measure of this unrest, as is the variety of proposals put forth to improve Congress's capacity to judge scientific programs. There is no doubt that Congress does have to make some adjustments to changing patterns of federal expenditure, and all the proposals deserve to be taken seriously. But before a wholly new system for dealing with science is created, it would be well to examine both the source of Congressional interest in science and the kind of advisory structure best suited to its needs.

There are at least three reasons for the interest of Congress in improving its grasp of science and technology. The first is cost consciousness—this year's federal R&D budget is about $15 billion. Congress is concerned, however, not only about the amount of money spent on research and development (which has multiplied 100-fold since 1940) but about the relationship of cost to performance. How can Congress make intelligent decisions when budget costs are based on estimates which fail to hold true? The Air Force, for example, estimated in 1960 that Project Skybolt would cost $893 million; in 1961 the estimated cost had reached $1.9 billion, and by the summer of 1962—when Skybolt was scrapped—not only had the cost estimate climbed to $2.3 billion, but Skybolt was a year and a half behind schedule. Another example is the project for the nuclear-powered airplane (ANP). In November 1951, one contractor estimated that it would take $188 million to deliver the nuclear power plant for mounting in an aircraft by May 1956. By 1961, when the project was cancelled, the costs of that one company had reached over $527 million and the power plant had never been delivered. The total cost of ANP, when it was ended, exceeded $1 billion.

It is true that the money supposedly "wasted" on the nuclear-powered plane may yet pay valuable dividends when some of its positive findings in metallurgy and instrumentation are applied to some future project, such as the supersonic airliner. Knowledge, however useless at the moment of its discovery, will someday find its place in the scheme of things and make its contribution. Nonetheless, a better way must be found to estimate the long-range costs of R&D programs; more accurate target dates for their completion must be determined. And Congress needs to be more accurately informed on both, not only for their implications for the budget and the sensible allocation of funds for R&D, but for their frequent implications for national defense as well.

Legislative Control

A second reason for Congressional attention to what Vannevar Bush has called the "endless frontier" is the belief among some members that Congress has lost the ability to oversee effectively the vast diffusion of R&D activities for

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The author, U.S. Senator from New Mexico, is chairman of the Senate Aeronautical and Space Sciences Committee, and is a member of the Joint Committee on Atomic Energy and the Interior and Insular Affairs Committee. This article is based, in part, on an address delivered 20 November to the Atomic Industrial Forum.

which it appropriates funds. As proof, take the statement of Senator E. L. Bartlett (D–Alaska) when he recently proposed the creation of a Congressional Office of Science and Technology: ". . . At the present time," he said, "the Congress does not appreciate the importance of scientific decisions and as a result they are made, not in the Halls of Congress, but elsewhere, not by the elected representatives but by unknown administrative officials. . . , How is a popular elected government to control its own activities? How are elected officials to direct development of something they do not understand with implications they do not comprehend?" These questions go to the heart of our representative system.

And, third, there is concern that the procedures of Congress may not measure up to the demands of "big science." New techniques for obtaining information may be required so that Congress will approach parity of knowledge with the executive agencies —in other words, that Congress will have its own sources of accurate information apart from the agencies and that this source of information will better enable Congress to judge the merits of any particular research and development project.

All these concerns are serious, and I certainly agree that Congress needs advice on scientific and technical matters. But before we go about setting up a system, it is important to clarify the definition of what "scientific advice" is, and to figure out what kind of scientific advice Congress needs.

"R&D" Distinction

Of the $15 billion of federal expenditure which too loosely gets labeled as spending for science, only $1.5 billion is for basic research. Another $1.2 billion is for research and development facilities; and $12.3 billion is for developmental hardware—not science, but engineering and technology. Most of this spending is accounted for by the revolutionary changes in defense systems which have taken place within the last decade. On these hardware items, engineers can give better estimates of cost and time than the producers of the scientific concept. The first point, therefore, is that advice on engineering must be included in the definition of scientific advice.

Another important consideration is that Congress needs to look on science not as an independent function such as agriculture or defense, but simply as a factor to be weighed in the solution of a variety of problems.

When Congressmen look at the test ban treaty, or water pollution by synthetic detergents, or the NASA authorization bill, they see issues of public policy on which decisions are made not alone, or even primarily, on the basis of technical factors, but also on many other considerations as well—administrative, economic, political, and social.

Ninety percent of the approximately $8 billion the Defense Department spends for research and development goes to produce hardware for better transport, communications, weapons, and other equipment to give the military the wherewithal to fulfill its approved missions. Knowledge of science and technology is not required for Congress to determine whether this money is being spent in consonance with assigned defense responsibilities. The executive examines in great detail the way in which the Defense Department should operate; the detailed justification of the Defense budget reveals to the congressional committees what is hoped to be achieved with the funds. This can be measured against congressional understanding of military missions. Congress, for example, is fully capable of determining the roles of the Air Force and NASA in the total space program. It can weigh the broad missions of the Air Force and how best to accomplish them. These are neither scientific nor technical questions.

The kind of advice and information needed by Congress varies. At times, particularly where the major factor is technical in nature, we need the advice of the most prominent scientists we can obtain. A discussion by knowledgeable scientists of the earth orbit versus the lunar orbit as the best way to get men to the moon, for example, would have helped us better understand the choices before us, the limitations of the alternatives, and the probabilities of success or failure. A panel, on occasion, could assist in reviewing a particular segment of an agency's program, such as the adequacy of NASA's provisions for space sciences or the basic research part of the defense R&D budget. But I doubt if Congress could usefully employ such eminent scientists full time.

Needed: The "Generalist"

What Congress needs most, it seems to me, is the advice of the well-rounded "generalist" who, having a scientific or engineering background, is familiar with the workings of the Federal Government, and with a number of executive agency R&D programs, particularly with their management. Experience in coordinating the work and projects of others in terms of the over-all mission or goal would be valuable He should be familiar with the scientific and technical community, so that he will know where to seek help when it is needed. He must have an appreciation of the values and ways of the legislative process, a feeling for public policy, and a capacity for sorting out public issues, competing values, and alternative solutions. Additionally, we need a person whose engineering background enables him to give us sound judgment on the costs of a project.

Proposals for a single source of advice to Congress do not take sufficient account of the committee structure. Each committee is restricted in interest and scope of responsibility, yet many areas of congressional interest cut across several fields. For example, we cannot really review water research and development, or oceanographic research, or total basic research, or scientific manpower resources, without cutting across committee responsibilities and looking at many executive departments. Likewise, scientific and technical advice is required from many disciplines. For Congress, or the Senate itself, to have a staff in a position to answer all of the inquiries of the various members and committees would require a duplication of the staffs within the executive agencies. It would require people with detailed knowledge of the missions and programs of all of the executive departments. This is impractical, it is too costly, and it has never been the intent of Congress. Furthermore, I do not think Congress needs it.

Filling the Need

I do not see how three or four scientists and engineers can provide even the Senate with the quality and quantity of advice needed by its committees. The demands on both their time and their talent would be too great. Further, the general terms, science and technology, need to be broken down into scientific disciplines before we can analyze what kind of scientists and engineers we are talking about and whether or not they could meet our needs. How could a biologist, a chemist, and a physicist, either separately or in combination, assist the Senate Committee on Aeronautical and Space Sciences in determining whether to authorize funds for a deep space probe or a specific type of communication satellite system? If the physicist has a space background which enables him to be useful to one committee, then his time would presumably be taken up with that committee work and he would not be available to other committees. Similarly, if the biologist were busy assisting the Committee on Agriculture, he would not be available to help other committees during the time when hearings were being held simultaneously by several committees. How can one biologist assist with problems of pesticides, the pollution of air, land, and water, manned spaceflight, or radioactive isotopes for cancer research? For that matter how can any single man be the repository of all relevant knowledge about his own discipline? Men who are experts in naval reactors are not necessarily qualified to advise even on reactors for space propulsion. If the function of these experts is only to put us in touch with other experts, I should like to point out that this is what our permanent committee staff is already doing.

In the last analysis it is the collective wisdom of Congress itself which counts most in making important decisions. No decisions can be made in isolation, on a completely scientific basis, by disinterested officials. Congress will consider the scientific aspects of a proposal and pay attention to the facts assembled by the engineer. But in addition, Congressmen must ask some further questions: What will the impact be on our economy? What effect will the proposal have on our foreign relations? Will it contribute to the health and welfare of the nation?

It is said that Congress, because it has maintained certain rituals for years, is a 19th century body faced with 20th century problems. I disagree. Precedents and practices of Congress may have been maintained that are perhaps archaic in this age of science and technology. But the minds of Congressmen are products of the 20th century. There is no relationship between the rituals maintained by an institution and its mental capability. Congress could legislate as well in the 20th century if its members still wore powdered wigs and capes instead of Ivy League clothes.

Congressional Initiative

There are numerous examples from the area of atomic energy when Congress spurred momentous decisions, in the face of inconclusive advice from experts, which have withstood the challenge of history and have proved right:

1) The decision to proceed with the development of the hydrogen bomb against the advice of the General Advisory Committee of the Atomic Energy Commission;

2) The decision to plan a broad weapons program which required the development of large quantities of fissionable materials, even though predictions were that this country could never provide the uranium-235 and plutonium needed;

3) The development of the *Nautilus* and the nuclear submarine fleet, against determined opposition;

4) The development of a variety of power reactors.

Early in the 1950's there was some discussion about the potential benefits of multiple purpose reactors. Coming from the arid southwest, I had some acquaintance with the problem of developing new sources of water for a rapidly growing population, and I found the possibilities quite fascinating. As a member of the Senate Interior Committee, I had had a hand in legislation accelerating the work of the Office of Saline Water in demonstrating techniques for converting brackish and sea water into potable water; I knew that the drawback of known conversion processes was that the expense of the large energy requirements for desalinization made the end product economically unattractive.

In 1955, in response to a request, I received a letter from a technical employee of the Los Alamos Scientific Laboratory outlining the potential benefits of a multipurpose reactor. I was not interested in, and certainly not qualified to judge, the "how" of the reactor. I was interested in the "why" of the concept, and whether we should invest in its development.

Reactor Proposal

The letter from Los Alamos, written in simple English, described a type of reactor with three different characteristics: it would produce electrical energy, it would breed more fuel than it consumed, and its by-product heat could be used to distill saline water. Clearly, there was a good deal of economic appeal in this. But reactor technology then was not up to the task. Some congressional prodding was required to get the AEC to move forward with studies of multipurpose reactors.

As a result of that prodding we will in time develop nuclear electrical energy at a cost of a 1½ or 2 mills per kilowatt hour and water at a cost of about 15 cents per 1000 gallons instead of the present cost of $1.25. This will be a practical result arising from the action of practical men urged on by scientists who are called in by a member of Congress for advice, but who do not become members of a congressional staff.

Perhaps this illustrates how a legislator can help shape—I hope intelligently—decisions on science and technology. The process of cross-pollination, exposure to a range of problems through various committee assignments, can supplement the advice of experts in helping Congressmen reach decisions. So can the process of osmosis, through which, over a period of time, members of Congress, through their committee assignments and awareness of the world around them, absorb some familiarity with the language and problems of scientists and technicians. Since science is only one factor in shaping the good society, I would paraphrase Clemenceau: science is too important to be left solely to the scientists.

I do not want to leave the impression that Congress has been infallible in its decisions on science and technology. Congress has made mistakes. In many cases, it has pushed programs too hard. But our scientific advisers have also made misjudgements. And we cannot count on one group to do the whole, difficult job.

Instead, we should try in a variety of ways to overcome the problems involved in the relationship of Congress with the "endless frontier."

1) We should strengthen the staffing of all committees which deal with science.

2) These committees should make intelligent use of *ad hoc* groups to give counsel on technical problems.

3) There should be an easier flow of information among the congressional committees themselves so that Congress avoids needless duplication in repetitious hearings and over-burdening of witnesses.

4) Representatives of the executive agencies should improve their method of presentation to congressional committees. In discussing purely scientific problems, there is no coloration of "executive" or "legislative" science. It is science for the nation as a whole. There are a limited number of people available with the broad knowledge necessary to give Congress advice on purely scientific questions. Although the Office of Science and Technology is an arm of the President, it would be most helpful if its staff could testify fully and adequately before congressional committees. The separation of legislative and executive powers in this regard can be carried to an extent that does damage to programs in which both branches have a mutual interest.

5) The channels for gathering information through the Legislative Reference Service of the Library of Congress should be expanded, and greater use should be made of such existing organizations as the National Academy of Sciences–National Research Council and the National Science Foundation.

6) Congress should receive an annual report on the state of science and technology. Each year we receive from the President a message on the State of the Union, a Budget Message, and various other reports. The President transmits to us through the National Aeronautics and Space Council a report on the year-long activities in space and aeronautics. Perhaps the National Academy of Sciences, through its various committees, could prepare a report by itself or in association with others such as the Office of Science and Technology. The report would briefly discuss the major programs in science and technology and would set forth what problems might be on the horizon which would require congressional attention. Separately, but more effectively, in conjunction with the National Academy, the National Society of Professional Engineers might report on the state of engineering since engineering is such a large part of government R&D programs.

There are no magic ways or easy devices to solve the problem of providing Congress with adequate advice on science and technology. Any approach that some would view as ideal would still be a long way from perfection and could also produce undesirable effects upon both science and government. As H. L. Mencken said: "An idealist is one who, on noticing that a rose smells better than a cabbage, concludes that it will also make better soup."

But those who are the doers of science, and we, in political life, have a mutual responsibility to improve the relationship of Congress and the "endless frontier." As concerned individuals and collectively as members of society, we have a stake in this task.

THE PRESIDENT'S SCIENCE ADVISERS

PHILIP H. ABELSON

The Role of the Adviser

TODAY and in the foreseeable future, a nation's prosperity and security depend upon the wisdom and timeliness of decisions made with respect to science and technology. The circumstances dictate that the best scientific and technical minds be consulted and that the results of their deliberations be conveyed promptly to the highest authority. Since the head of a government has a wide spectrum of responsibilities, can work closely with only a limited number of persons and is himself no expert in science and technology, it is not uncommon to maintain the post of Science Adviser and to use its incumbent as the primary source of counsel in scientific and technological matters. In the United States the President, since late 1957, has had frequent recourse to the services of a Science Adviser.

As with all human arrangements for the exercise of power there are advantages and disadvantages in concentrating authority for decision-making in one person. The office and its supporting machinery are relatively new and correctable deficiencies are to be expected. In what follows I will largely portray some negative features of the advisory complex.[1] My negative approach is due in part to the fact that it is easier for an outsider to identify failures than to specify successes. There is a paucity of positive evidence concerning the quality of performance of the President's advisers, especially the last two. The major tangible basis is a relatively thin collection of reports arising from studies conducted by panels appointed by the adviser.

As an illustration of the difficulty of identifying contributions of the Science Advisers, consider events surrounding the Nuclear Test Ban Treaty. Dr. Wiesner, who was Science Adviser at the time, was known to favour such a treaty. However, he was one among many proponents. In the important Senate hearings some 40 witnesses testified. He was not among them. Nor did he participate in the crucial negotiations at Moscow.

There is little public evidence that the adviser or his staff have addressed themselves to many major problems which might be expected to fall within the adviser's responsibility. For instance, a crucial problem is how to harness effectively the vast scientific and technological resources of the nation. Do the present allocations of money and manpower make sense? If this is too big a question, an alternative one is: do present policies of research support at times damage the national interest? Some of us believe they do.

The advisers and their staffs have concerned themselves with many relatively trivial problems to the annoyance of some of the other parts of the government. Anyone acquainted with Washington civil servants engaged in science can learn of their discontent with the advisory system. Many such scientists have felt arrogantly mistreated by the men who have occupied the post of Science Adviser or by their agents. In what follows

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[1] For a favourable treatment of the adviser's role, see special report on "White House Superstructure for Science", *Chemical and Engineering News*, XLII (19 October, 1964), 42, pp. 78–92.

I will discuss the power structure available to the President's Science Adviser. I will then describe how the structure has at times functioned and make some suggestions for improvements.

The Science Adviser's authority stems from many sources. He is the President's personal science adviser, the Chairman of the President's Science Advisory Committee, the Director of the Office of Science and Technology, and the Chairman of the Federal Council on Science and Technology.

A major element in the power structure is the Office of Science and Technology. The formal objectives of the creation of this organisation can best be described by quoting directly from the President's Message to Congress of 29 March, 1962.

> . . . it is contemplated that the Director will assist the President in discharging the responsibility of the President for the proper coordination of federal science and technology functions. More particularly, it is expected that he will advise and assist the President as the President may request with respect to:
>
> (1) Major policies, plans and programs of science and technology of the various agencies of the federal government, giving appropriate emphasis to the relationship of science and technology to national security and foreign policy, and measures for furthering science and technology in the nation.
> (2) Assessment of selected scientific and technical developments and programs in relation to their impact on national policies.
> (3) Review, integration, and coordination of major federal activities in science and technology, giving due consideration to the effects of such activities on non-federal resources and institutions.
> (4) Assuring that good and close relations exist with the nation's scientific and engineering communities so as to further in every appropriate way their participation in strengthening science and technology in the United States and the Free World.
> (5) Such other matters consonant with law as may be assigned by the President to the office.

To implement an effort to coordinate research, the Science Adviser has been given major additional power. He participates in decisions on budgetary matters. In effect, he is at times director of the Bureau of the Budget where scientific matters are concerned. Such a statement could be made on the basis of conversations with government people in Washington but the matter has been described fairly clearly in an address by William D. Carey, Executive Assistant Director of the Bureau of the Budget. His statement was printed in the *Congressional Record* of 30 September, 1963, from which the following is quoted:

> The Bureau of the Budget has never agreed with suggestions that it should establish within its structure a Division of Science, staffed with qualified scientists and engineers, to review R. and D. proposals. To be sure, our analysis frequently requires input of sophisticated professional judgment as to technical feasibility, state of the art, and possible alternatives to a proposed line of development, as for example in the moon program or in the missile field. In recent years, however, we have been able to obtain this kind of judgment through the Office of Science and Technology and the President's Science Advisory Committee. This year, for example, we selected the areas of atmospheric science, oceanography, water research, high energy nuclear physics, basic science, and science information for special review, and we conducted this exercise jointly with the Office of the President's Science Adviser.

It is clear from this quotation that the Bureau of the Budget is inclined to listen to the Science Adviser's counsel on scientific matters. Should some question arise as to his rightful pre-eminence, he has at his disposal a battery of leading scientists who would be stronger than any group which the Bureau of the Budget might assemble. I need not dwell on the real power that monetary control gives. Even the most powerful agency head must worry about his budget. I know of no evidence that this budgetary power has been misused but its very existence cannot but colour relations between the adviser and various agencies of the government.

Another unofficial source of power is activity in the appointments of top-level scientific personnel. When an important post becomes vacant, the Science Adviser, among others, is consulted in filling it. In view of the various positions he formally occupies he has a considerable voice in the making of such appointments.

Adviser and the Advised

It seems evident that the Science Adviser, whoever he may be, has available an ample machinery of power. The manner in which the structure functions, however, depends crucially on human factors—among them the temperament of the President, the relations between the President and his Science Adviser and the temperament of the adviser. For instance, President Kennedy displayed considerable interest in science and the President's Science Adviser had more and easier access to Mr. Kennedy than any other scientist had. In the *New York Times* Magazine Section of 3 September, 1961, Mr. Finney has said:

> There is also a close, informal relationship between the President and the young engineer who was one of the campaign confidants and advisers. Hardly a day passes that Wiesner does not talk to the President, either in person or by telephone.

Such a relationship does not go unnoticed in Washington, where agency heads and Congressmen are sensitively attuned to nuances. Access to the President gives a man power that others respect and defer to. Close association with a President carries with it liabilities as well as assets. Presidents are usually intensely political in their outlook and they tend to react to events as they occur. In the process they urgently use whatever tools are at hand. A Science Adviser enjoying the complete confidence of a President may find himself diverted to fighting a series of minor political brush fires. He must also be responsive to any special interest, however misguided, that a President may evince in scientific or technological matters. While thus occupied, the adviser must give secondary priority to longer-term problems which may be of much greater enduring significance.

The relationship between President Johnson and his Science Adviser, Dr. Hornig, has naturally not been very close. Dr. Hornig was designated by President Kennedy and before the assassination Dr. Hornig and Vice-President Johnson had little contact. During most of 1964 the President was heading what was necessarily an interim administration. With a fresh mandate, he became free to choose his own advisers and to operate in ways that suit him best. Regardless of whom he chooses as the adviser, be it Dr. Hornig or another, a new relationship will evolve.

One factor that will shape that association will be the demands of the times. If the cold war eased further, there could be a lessening of the need of the President for an adviser. If the economy were to slow down, efforts might be made to use science and technology as a means of creating new jobs. The long-term trend is towards a larger role for science and technology in most aspects of civilisation, which is likely to lead to an increase in the importance of the role of the Science Adviser.

The mode of conduct of the Science Adviser's establishment is considerably influenced by his own personality and especially by his appetite for participating in many decisions. In the course of their activities the Science Advisers may, if they wish, make recommendations concerning a wide variety of problems.

The Adviser and his Advisers

To help him the President's Science Adviser has in addition to an operating staff the benefit of two principal advisory mechanisms. The most prestigious of these is the President's Science Advisory Committee (P.S.A.C.), of which he is chairman. This committee consists of 18 distinguished scientists who customarily meet for two days each month. In the meetings an agenda of numerous items is covered. In situations like this, a full-time chairman dominates the proceedings as far as he wishes. He can select the items that appear on the agenda. He can choose the information to be presented to the committee, and even the individuals who will perform the necessary briefings. With committees of this type it is only necessary for the chairman to be able to count on the active support of four or five of the 18 to assure effective control. Members of this committee serve for four years and are appointed by the President, who, in making appointments, naturally consults the man who is both his Science Adviser and the chairman of the committee. Even a man with moderate talents could control such a situation.

The adviser has at his disposal, if he wishes to use it, essentially the total scientific capabilities of the United States, for almost everyone will respond to a request from the executive offices of the President. The usual method of employing them involves *ad hoc* panels. As an example of how a panel might work, consider the two hypothetical questions about the National Aeronautics and Space Administration's policy with respect to scientific exploration of the moon. Should all unmanned vehicles concentrate or obtaining information relevant to the Apollo programme? Should scientific experiments be conducted that apparently have no direct relevance? To answer such hypothetical questions, a panel of scientists would be convened. The executive secretary of the group would be the man who is in charge of the space desk in the Office of Science and Technology. One or more members of the panel would be members of the President's Science Advisory Committee. Others would be distinguished scientists who had participated actively in the space programme.

There are those who might inquire as to the justification for the President's Science Adviser's participating in a policy matter so central to the activities of N.A.S.A. The answer is, of course, that lunar exploration requires money and, in turn, decisions by the Bureau of the Budget.

No official list has been published which permits complete identification of panelists, but the general composition of the panels is known, in part through the work of Miss Greenfield of *The Reporter* magazine, who spent a year studying the operations of the Science Adviser's Office. She states that "about half the mystery guests come from private industry".[2] A substantial number of the remaining half, the academic representatives, are either at present in the Cambridge area or have in the past had some connection with it, such as service in the M.I.T. Radiation Laboratory during World War II.

Actually, the panel system has serious potential weaknesses. Brilliant men come to Washington for a few days, are given detailed briefings, and then, with a limited amount of time for consideration, form opinions on matters that may involve tremendous sums of money and even the security of the nation. Their eventual recommendations can be manipulated by controlling the nature of the briefings. Their report, when issued, is in general not a public document. Thus, if it is not convenient to implement the conclusions, the report can be pigeonholed.

Advisory groups such as P.S.A.C. and the panels can be a valuable source of ideas and information but they cannot usefully make operating decisions, nor can they implement them. Since the panel members play an advisory role and since they are stationed elsewhere, they cannot be held fully responsible. They have considerable influence while at the seat of power, but once away from it, their influence diminishes.

Numerically, the 300 or so panel members who have been called upon by the adviser represent on the order of 0·01 per cent. of the nation's scientific and engineering brains. Are they our best brains? We have no way of knowing, for the identity of their owners is kept secret. Are they used effectively, or are the advice and ideas of these men circumvented? We do not know. Could the ideas involved be sharpened by suggestions and knowledge possessed by the 99·99 per cent. who are not consulted? Again, we do not know because it has not been tried.

The Adviser and his Staff

With all his duties the Science Adviser can personally attend to only a tiny fraction of the problems and developments. Although he can get much good counsel from advisory bodies, he must depend heavily on a group of full-time associates. The principal source of assistance is a staff of professional aides totalling about 16.

An official version of what this group does was presented in a statement by a former adviser on 16 October, 1963, at a hearing of a subcommittee of the House Committee on Science and Astronautics[3]:

> The Office of Science and Technology, since its establishment last year, has developed into a staff mechanism to serve the Special Assistant, the P.S.A.C., and the Federal Council. Slightly more than a dozen highly trained specialists in science, engineering and administration provide

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[2] Greenfield, M., "Science Goes to Washington", *The Reporter*, XXIX (26 September, 1963), 5, pp. 20–26. The article was reprinted in *Science*, CXLII (18 October, 1963), 3590, pp. 361–367.

[3] Statement of Dr. Jerome B. Wiesner at Hearings on Government and Science before the Subcommittee on Science, Research and Development, House Committee on Science and Astronautics, 88th Congress, First Session (16 October, 1963), pp. 58–109.

> assistance to these activities by identifying issues, phrasing questions, gathering information from the agencies on problems in scientific fields, working closely with the Bureau of the Budget, the Council of Economic Advisers, and other Executive Office agencies, and following up the implementation of federal programs. In addition, the Office has served as the channel by which the Executive Branch meets its duties to the Congress on matters of over-all science policy.

That the Office of Science and Technology staff has large responsibilities and authority is indicated in the phrase " following up the implementation of federal programs ". One would expect this staff to be of the highest professional calibre. I was, therefore, interested during October 1963 in checking up on the qualifications of this group of so-called " highly trained specialists ". Although a few were found to be competent, they did not in sum constitute a distinguished group of scientists. A search of *Chemical Abstracts* and *Science Abstracts* revealed that during the past 10 years few of the 16 men had written for any of the publications abstracted by these services.

Nevertheless, this group collectively has exercised perhaps almost as much power as the adviser. Some of the mechanisms by which power has been exerted have proved distressing to government agency heads. For example, the director of a great national establishment, often a man of outstanding scientific stature, might receive a telephone call. The voice at the other end would say: " This is the White House calling ". The director was faced with a dilemma. Whom did the call represent? Was the Science Adviser's Office calling to pass on an order from the President, or the President's Science Adviser, or the Chairman of the President's Science Advisory Committee, or the Director of the Office of Science and Technology, or the *de facto* Chief of the Bureau of the Budget? Or was the staff member from the Office of Science and Technology who was making the call simply throwing his weight around? In the past the last has often been the right interpretation but the agency head felt well advised not to question too closely, even though he might suspect that he was being bullied by a minor member of the staff. Not infrequently the call was a request for charts and information to be used in a briefing the next day. The director had no alternative but to turn his agency upside down to meet the deadline. If the President or the Science Adviser were personally interested in the material and he failed to deliver, he might find himself in a highly embarrassing position.

On a number of occasions I have been in touch directly and indirectly with many of the major agencies in Washington. Almost all have had numerous calls from staff members in the adviser's office. It was not always the agency head to whom the request was directed. I am told that members of the staff of the Office of Science and Technology have seen fit to communicate with subordinates within agencies, sometimes without the knowledge of the responsible head. This kind of procedure can wreak havoc with morale, for when it occurs, the underlings often assume that their chief is on the decline.

About a year ago, in talking with a man relatively high up in the Space Administration, I elicited the information that the man who has the space desk at the Office of Science and Technology was in frequent

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communication with various subordinates in the Space Administration and that he often concerned himself with technical matters. N.A.S.A. has attempted to obtain the advice of the scientific community and for this purpose has a number of advisory groups whose membership includes Nobel Prize winners, many members of the National Academy of Sciences and most of the leading contributors to space research, such as Professor James A. Van Allen. The total membership of these advisory subcommittees is about 70. In an effort to assess the weight their advice carries, I asked the official this question: "Would you say that an underling in the Office of Science and Technology has more weight in technical decisions affecting the Space Agency than that possessed collectively by all your consultants?" The reply was, "Yes, he does." The official in question is a conscientious person, and I would not wish to seem to disparage his character or accomplishments. But the fact is that he has not demonstrated unusual proficiency in the area in which he wields so much authority.

According to the original plan, staff members of the Office of Science and Technology were to act as subordinates, making arrangements for panels and acting as go-betweens in the transmission of information from the adviser and the President's Scientific Advisory Committee to the agencies. In practice, however, P.S.A.C. and the panel members are on hand only a small part of the time. The adviser is engaged in hundreds of activities. Someone must make interim decisions and the responsibility naturally falls to the staff of the Office of Science and Technology. Thus, in reality, though it was not planned that way, a major fraction of government research policy is made and executed by a small group of men having little proven eminence or even competence in science.

The Adviser's Jurisdiction

My impression is that many of the unsatisfactory features of the adviser's conduct of his office in the past have arisen from the attempt to deal with too many of the wrong kinds of problems. It would be far better to do a few supremely important tasks well than to be immersed in a bottomless morass of petty detail. There are important situations which from their very nature must be handled at the top level. Matters like the nuclear test ban and first-rank military questions encompassing sophisticated science and technology probably must be dealt with at the highest level. Then there are issues such as that raised by Rachel Carson's *Silent Spring*[4]; if only for political reasons, a President would feel obliged to employ high-level advice in making a response to the furore raised by such a book.

There are other problems with which the Science Adviser should grapple and for which his office is uniquely fitted. Problems involving long-term planning are an example. In the United States remarkably little attention has been given to the future and to the consequences of contemporary activities. For instance, our research grants system is profoundly affecting higher education, probably adversely, yet the trends go almost unnoticed.

Another class of problems are those transcending the competence or jurisdiction of any existing arm of government and requiring the ultimate establishment of new mechanisms. A good example is the need that arose for a new agency to take jurisdiction over space matters. The first Science

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[4] (New York: Houghton Mifflin Co., 1962; London: Hamish Hamilton, 1963.)

Adviser, Dr. Killian, was instrumental in the creation of the National Aeronautics and Space Administration.

The present structure of the adviser's office has at least one glaring defect, which is a consequence of the formal objectives of the Office of Science and Technology quoted earlier. Some of the relevant phrases include: " the Director [Science Adviser] will assist the President in . . . the proper coordination of federal science and technology functions. . . . he will advise and assist . . . with respect to . . . (3) Review, integration and coordination of major federal activities in science and technology . . . ".

The periodic review of major programmes can be extremely useful, provided that such examinations are conducted in depth, with elaborate care and due dignity. The best brains of the nation can be recruited for such a task. This is the kind of effort the scope of which can be delineated precisely and the proceedings can lead to greater effectiveness in relevant agencies and at times to the initiation of new procedures or even to useful legislation.

In contrast the words " integration and coordination " carry with them no end of mischief. In their present context, the words are fuzzy, even utopian, in connotation. It is literally impossible to coordinate perfectly the activities of two creative scientists—let alone thousands. Thus the Office of Science and Technology is chartered to perform a task that can be neither sharply delineated nor actually accomplished. Moreover, since new developments are always occurring, the unattainable goal changes constantly.

In the long pull the best brains of the nation are too astute to be trapped into such an undertaking. If " integration and coordination " are to be performed by the Office of Science and Technology the burden must rest with the limited staff of the office.

On 22 November, 1963, there appeared in *Science* an editorial written by myself which seems to have continuing validity, and which may serve as a conclusion to the foregoing reflections on the problems of organising the system of scientific advice in the United States:

> In his many roles Wiesner has been required to present at least three differing visages to the world. As the President's adviser, his appropriate function has been that of self-effacing, impartial judge, often acting under tightest security. As director of O.S.T., it has been in the public interest for him to wield authority as openly as possible. His power also implicitly has required him to be a statesman of science—a deep thinker—with long-term views of evolving patterns in science and technology and of the relations among society, science, and education.
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> The realities of politics and power dictate that the role of President's Science Adviser and its needs should transcend all other functions. Almost inevitably the secrecy necessary to that office has been carried over into the Office of Science and Technology, which attempts to keep secret even the identity of its . . . consultants. The realities of human behavior also dictate that immediate operating decisions take priority over long-term thinking.
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> . . . Means should be found to separate functions of the P.S.A.C.-O.S.T. complex into logical packages, with no one man asked to perform more than is humanly possible. The job of President's Science Adviser is a big one which merits full-time effort. A full-time director should lead O.S.T., and he should have a far better staff. Finally, we need a Planning Office headed by a man who can think and who can marshal the wisdom of this nation in attempting to give guidance for the future.

National Planning for Medical Research

Philip Handler

Let me say at once that I do not advocate the synoptic planning attempted by the systems analysts, or the balanced growth which is frequently taken as a prime desideratum, but, instead, recommend that those who plan a national medical research enterprise exercise skillful opportunism as they stimulate the growth of the system by relatively disjointed increments.

At first approach such planning seems simple. A small nation with limited resources of funds, facilities, and manpower need merely decide which is the most important biomedical problem in its part of the world and then direct those resources to solution of that problem. An economically well-developed nation, with substantially greater resources, might consider simply giving the entire system free rein in the expectation that its scientists will attack those problems which are important and approachable experimentally. Later, in retrospect, one might assess what had actually been accomplished. But neither approach is really acceptable. All the considerations which have been raised with respect to the allocation of some fraction of a nation's total resources to the biomedical research enterprise are equally appropriate when one attempts, in turn, to fractionate that enterprise. Accordingly, the problems posed by biomedical research in the smaller or less-developed nation *are* more simply managed than are those of the more complex nations. One cannot but feel that control of schistosomiasis or of frank malnutrition, for example, where these are endemic, is of overriding importance. Surely these much more properly command the attention of those concerned with the public health in such areas than do the more universal problems of heart disease, cancer, or genetic disorders.

For those responsible for decisions under such circumstances, I have but one counsel. Every research enterprise flourishes best when the group which is so engaged attains some meaningful, critical mass. Hence, a nation with one or two medical schools should seriously consider the possibility of developing only a limited number of research groups, each addressed to a problem of maximal concern to that nation and each large enough and so equipped and financed as to afford some prospect of success. Such success will not only have immediate relevance to the public health of the area but will effect a marked enhancement of morale and create an intellectual and political climate of richer opportunity for subsequent endeavors.

Only a handful of major clinical triumphs, such as the eradication of pellagra, penicillin therapy for syphilis, general antibiotic therapy, treatment of arthritis with steroids, and the recent accomplishments of vascular surgery, have, in the United States, served as catalysts which have opened the public purse for support of biomedical research. Those nations which, of necessity, can at present expect to mount only relatively more modest biomedical research enterprises may find it best not to engage competitively in those aspects of medical research which are under intensive investigation elsewhere. I do not mean to imply that individual scientists in smaller nations cannot successfully compete, for example, in molecular biology. Nor do I suggest that the scientists of the emerging nations must mark time for decades as they retrace from its beginnings the long evolution of medical research. Quite the contrary. The scientist born in one of the emerging nations but trained in one of the older laboratories and with access to current literature need suffer no handicap save the limitations of his own talent and of the resources which his society places at his disposal. Nevertheless, unless he can be joined by a sufficient group of competent colleagues, I believe he will best serve his own ends and those of his nation by addressing himself to a problem of unusual significance in his own locale.

Internal and External Pressures

The 2nd NIH International Symposium on Biomedical Research has emphasized the concept that, for science generally, two significant sets of pressures determine the allocation of resources: pressures which arise from within the scientific community and those which arise from without (*1*). This concept is equally applicable to the allocation of resources within the biomedical enterprise. The pressures from without are easily identifiable. They include the general aspiration to free man of cancer, of heart disease, of infection, of malnutrition, of fears in the night; society expects, and quite rightly, that much of the total research effort shall be directly devoted to these ends. They include the expectation that the biomedical community will operate an educational system which will produce physicians in sufficient numbers to provide adequate care for all members of society. They include the expectation that those engaged in research will reproduce their kind in numbers sufficient to assure an adequate continuing supply of individuals who will pursue medical science. And it is gratifying to recognize that they include a growing expectation that man will intensify not only his exploration of the universe in which he finds himself but his exploration and understanding of himself.

The internal pressures, generated by the research community itself, are less widely experienced but, unless modified, more likely to give direction to

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the conduct of research. For example, if left to its own devices, a substantial segment of the biomedical community is likely to eschew the immediate problems of disease. Some may enjoy the esthetics of enzyme kinetics, while ignoring metabolic disease; others may explore viral genetics, while ignoring the consequences of viral infection. Or, some wisp of the Zeitgeist may lead many to examine the mechanisms of carcinogenesis while none seek insights into the bases for schizophrenia. In sum, the scientific community continues to press for the vitality and expansion of the relevant scientific disciplines and for biological research at its most fundamental levels, preferring to defer direct attack upon overt disease until, in its view, the stage has been adequately set. In general, I share this approach.

It is the obligation of those charged with the responsibility for what is euphemistically called "planning for science" to be aware of both types of pressure, to admit that each is a valid criterion for decision making, and to recognize that neither set of pressures, alone, constitutes a sufficient basis for national decisions.

The Extreme Views

To be sure, each extreme view has had its exponents. At one extreme are statements such as that by Michael Polanyi (*2*), who argues, "No committee of scientists, however distinguished, could forecast the further progress of science except for the routine extension of the existing system. The pursuit of science can be organized, therefore, in no other manner than by granting complete independence to all mature scientists. The function of public authority is not to plan research but only to provide opportunities for its pursuit. To do less is to neglect the progress of science. To do more is to cultivate mediocrity and waste public money." The adherents of views such as this are numerous, and history documents their claims. Indeed, in only a handful of instances has organized society recognized a major problem and directed to it the scientists who found an appropriate solution.

In this country, for example, our Public Health Service recognized the threat posed by pellagra in our Southeast and dispatched Joseph Goldberger to investigate the problem. His triumph is now history, but it is rather ironic that, having prejudged the nature of the problem, the Public Health Service dispatched a bacteriologist to address himself to what proved to be a nutritional problem. And if this tale has any moral it is that the triumph reflected the genius of the investigator rather than the wisdom of those charged with allocating the then meager resources of the U.S. Public Health Service. How many instances of societal planning of successful major advances in the elucidation of human biology or in the understanding, prevention, or treatment of disease can one add to such a list? The development of Atabrine, understanding of the etiology of retrolental fibroplasia, the development of antiviral vaccines, and control of insect-borne diseases are among the relatively few such major, planned accomplishments. The development of new drugs by the laboratories of the pharmaceutical industry, an arm of organized society, must also be included.

On the other side of the ledger—that of the unplanned accomplishments which we owe entirely to the imagination and initiative of individual investigators—is virtually every other major advance in man's understanding of himself and of the disorders to which he is subject. Surely this history indicates that the criteria for research support which arise from within the scientific community are generally valid. In fairness, however, let it be said that large-scale public support of research and the opportunity to "plan" are recent phenomena, and this judgment must be held in abeyance.

Nevertheless, many concur with Hogben (*3*), who said, "To get the fullest opportunities for doing the kind of work which is worthwhile to themselves, scientific workers must participate in their responsibilities as citizens. Among other things, this includes refraining from the arrogant pretense that their own preferences are sufficient justification for the support which they need. This pretense, put forward as the plea that science should be encouraged for its own sake, is a survival of Platonism. Science thrives by its applications. To justify it as an end in itself is a policy of defeat."

Such statements engender much controversy—and properly so. Patently, modern society supports the laboratory of a scientist not so that he may amuse himself but, rather, in the hope that his activities will, in some measure, make possible realization of one of society's own expectations. To be sure, these expectations include, broadly, the advancement of knowledge, but this ranks well below the hope that the scientist's findings can soon be translated into some practical end. Accordingly, in this country we have attempted to manage a national enterprise which provides opportunity both for the scientific giants whose research, freely undertaken, results in "quantum jumps" in our understanding and for those scientists who seek to exploit such understanding in the common interest.

In our own time it has become apparent that planned science—here I use the term *planning* rather broadly—*is* feasible. There have been no planned breakthroughs, nor are there likely to be any. But there can be and there has been planned exploitation of such breakthroughs. Not even Fleming planned his astute observations, but the subsequent effort required to produce penicillin and to determine its structure was most effectively planned. Society did not plan Enders' observations of viral propagation in animal tissue in culture, but society did plan the large program which supported the development of effective antiviral vaccines. Society did not plan the observations which led to the strong suspicion that elevated concentration of serum lipid is related to the development of atherosclerosis and myocardial infarction, but society can and does plan the effort necessary to validate that conclusion and to develop means for alleviating this disorder. Watson and Crick were free scientists, engaged in a problem of their own choosing, but society could and did plan to support the broad-scale effort required to amplify their hypotheses, in the hope of bringing understanding of those phenomena which underlie genetic disorders of man, viral infectivity, and perhaps cancer.

But the administrators of science must not plan the *doing* of science. They can but plan *opportunities* for the doing of science and hope that talented, competent investigators will avail themselves of such opportunities. Effective planners may not do less and should not do more.

Planning a Research Enterprise

It becomes apparent that, in attempting to plan a national biomedical research enterprise, one must view the enterprise while simultaneously considering each of a series of seemingly independent parameters. Among these are the various diseases which ravage mankind, perhaps the organ systems of which man is built (liver, kidney, brain, and so on), the continuing vitality of each of the related scientific disciplines, and the integrity of the academic institutions in which much of the research is to be performed. One must weigh the relative importance of research done on man himself and research performed on animals or model systems; of research in the laboratory and research in the field; of research in areas clearly identifiable as "biomedical" and research, essential to an understanding of life, in tangentially related disciplines; of the support of research and the support of training for the future conduct of research; of the support of research and research training and the support of education in clinical medicine; and of hosts of seemingly lesser parameters. Each of these parameters is relevant to each decision concerning the planning and funding of individual research programs.

At this point one might visualize the development of a matrix in which each parameter has a weighted value and is brought to bear on each decision; this would be an idealized version of the approach of the operations or systems analyst. Successful development of such a matrix would seem to suffice for the total planning operation, and all one would then need to know would be the total appropriation to be made available by the state in any one year; all other decisions would then be automatic. This is an exaggerated version of what Charles V. Kidd has termed "allocation in multiple dimensions." In the exaggerated form here presented it is rather horrendous to contemplate and, no matter how conscientiously or painstakingly developed, is guaranteed to yield many decisions which time will prove to have been incorrect.

In a limited sense, however, the principle does have merit. Those encharged with planning responsibility must indeed be aware of the various criteria which are meaningful in the decision process. They must assure society that none of the meaningful parameters have been neglected, although they cannot possibly guarantee that a perfect balance among them all has been assured. Indeed, such balance is not even necessarily desirable.

Happily, in the real world, matters can proceed more easily and more successfully than the novice in planning might have thought. No nation has actually engaged in such detailed allocative planning. In most instances planning has been done, rather, in a single dimension. Resources have usually been allocated by disease or by discipline, or, in nations with university grants systems, have simply been apportioned among universities and other appropriate institutions. But for our purposes it is important to note that the other dimensions do exist, whether they are planned for or no. Each research project which is supported, or for which support has been denied, has relevance in virtually every possible planning dimension. And, in annual retrospective examination, it is imperative that the operation of the system be examined in as many dimensions as possible, so that, if necessary, corrective action may be taken. One can hope in this way to assure that certain broad priorities are operative. Probably highest among these is the assurance that, at all times, a future generation of investigators is being trained and that their number bears some reasonable relationship to the desired future magnitude of the national research enterprise. Second priority might be given the assurance that all the disciplines currently meaningful on the biomedical scene are given sufficient support to assure a vigorous national effort. Third priority might relate to the vitality of academic institutions and of individual laboratories. In fourth place might be the distribution of resources by disease categories, ranked in the order of the severity of such disorders in a given community. The fact that it is this fourth priority which is frequently given most obvious expression relates to political considerations rather than to the internal logic of the system.

It will be evident that in a nation confronted with a planning problem of this magnitude there *already* is a system in being which can be retrospectively examined and corrected. Indeed, much of what is called planning is essentially remedial in that it seeks to rectify apparent errors rather than move toward planned objectives. Planning proceeds from an existing base, and each proposed increment to the existing system can be considered rather readily from the multidimensional standpoint.

Allocations and Adjudications

These thoughts, lead, then, to consideration of the actual process whereby one establishes allocations within a budget and then adjudicates the competing claims of individuals or institutions within some category of that budget. Patently, this cannot be done in an information vacuum. The establishing of allocations is the more complex task, as it demands a weighing of the values of the internal and external pressures. These pressures certainly vary among nations, and in any one nation they must vary from time to time. In any case, they can only be designated as weak, strong, or paramount. Thereafter one requires real data descriptive of opportunities: knowledge of the number of competent investigators interested in a given area, of the physical facilities, of the number of students in training, and of the cost of doing business in a typical research group; and, most importantly, an assessment of the "state of the art" in each subfield of research endeavor—that is, an informed guess concerning when the time is right, conceptually and technologically, to increase significantly the level of effort in a given research area. Evaluation of this information and appraisal of the scientific field should permit tailoring of the demands of the scientific community to the interests of society. They yield a crude determination of the relative magnitudes of support to be given, for example, to fellowship programs, arthritis or dental research, genetics or pathology, clinical or basic research.

Such considerations are particularly germane to those components of the system which are properly called "small science"—science in which the individual professor or senior investigator and his coterie of junior colleagues are the meaningful productive and budgetary unit. Whether he works in a government-operated establishment or in a university where his work is supported by a national research grants program is inconsequential. When the funds available are less than those requested by the scientific community (and this should always be the case, else excessive funds have been provided), competing requests can be evaluated only on the basis of intrinsic scientific merit—that is, the competence of the investigator and the imagination, soundness, and feasibility of his proposal. The evaluation can be made only by a jury of his peers, drawn from a national panel of experts. To be sure, they may share his enthusiasm for his discipline but they are not rivals, on his local scene, for prestige, salary, space, or influence. It is the lack of this evaluative process which is the cardinal weakness of a university grants system and of other purely bureaucratic administrative devices. Conversely, it is the operation of this evaluation system which is the best guarantee that society will get its money's worth.

Proposals for "big science" are rare in biomedical research. They must be examined closely both for their intrinsic value and for the harm they could do the rest of the system through imposing a drain on manpower, facilities, or funds. By and large they are foreign to the university biomedical community, and, if they are desirable at all, their operation is a proper function of government or of a contractor-agent.

The greatest advantage of incremental planning is the fact that such planning makes it possible to seize previously unforeseen opportunity. And it is here that the quasi-mathematical approach to total planning fails most seriously, since it does not take into account the manner in which science itself grows. Let us consider this in some detail.

Balanced and Unbalanced Growth

There is a great temptation for those engaged in planning to attempt to project systems of "balanced growth." Indeed, "balanced growth" has been the acknowledged objective of most of those who plan a nation's economy, its weapons systems, and its support of science generally as well as its support of biomedical research. Although planners frequently recognize that they cannot realize this ideal, this so-called balanced system is the proximate objective of their development programs. As noted by Hirschman and Lindblom (*4*), the basis for this ideal is a "faith in the existence of basic harmonies similar to the Greek belief that the truly beautiful will possess moral excellence as well." It seems opportune, therefore, to direct to your attention a recent series of papers which have taken striking exception to the concept of planning balanced growth of a large enterprise and have advocated in its stead a process which has been called "disjointed incrementalism."

Because the analogies are pertinent to the problems here considered, it seems appropriate to summarize the views of various members of the group who advocate this process. For example, Hirschman (*5*), an economist, has offered as the basic defense of *un*balanced growth the concept that an economy's resources should not be considered as rigidly fixed in amount. He argues that more resources or factors of production will come into play if development is marked by sectoral imbalances, since these will arouse private entrepreneurs or public authorities to action. In the present context, there are many analogies. For example, the existence of a large pool of investigators who lack facilities for their activities constitutes a pressure which, ultimately, will result in the construction of new and more adequate facilities. The appearance of large numbers of young men and women desirous of training in biomedical research results in pressure which leads to the development of fellowship and training programs. Recognition that a temperate bacteriophage can disappear into the genome of the host bacterium, be reproduced with that genome for many generations, and then reappear in vast numbers under adverse circumstances prompts many investigators interested in the nature of the viral origin of cancer to take a new tack in their explorations. As Hirschman has said, to the extent that the imbalance is self-correcting through a variety of mechanisms, unbalanced growth may propel the economy forward jerkily but also more quickly than by planned, balanced expansion

Klein and Meckling (*6*), students of development policies for weapons systems, allege that a given development is both less costly and more speedy when marked by duplication, confusion, and lack of communication among people working along parallel lines. They argue against early attempts at integrating subsystems into a well-articulated, harmonious general system. They advocate, instead, the full exploitation of fruitful ideas regardless of their fit to some preconceived pattern of specifications. The principal basis for this attitude is the very fact of uncertainty. They note that the final configuration to be developed is, in any case, unknown, and that knowledge increases as some of the subsystems become articulate. Knowledge about the nature of any one subsystem increases the number of clues concerning the desirable features of another, just as it is easier to fit in a piece of a jigsaw puzzle when some of the surrounding pieces are already in place. What is important is to develop the pieces; one can adjust them to each other later. This view argues for maximum support of the current enthusiasm for molecular biology even though its immediate clinical application seems remote, and for vigorous follow-up of clues to the possible viral pathogenesis of cancer even though the major psychoses remain enigmas and relatively few biologists seem to be immediately concerned with their elucidation. Similarly, it argues for full support of all the competent scientists in our midst, even though this results in overcrowding of their laboratories.

Lindblom (*7*), who has been concerned with general aspects of policy making, takes as his point of departure a denial of the general validity of an assumption which is implicit in most of the literature on policy making—that there exists sufficient agreement to provide adequate criteria for choosing among possible alternative policies. This assumption is often questioned in contemporary social science, yet many of the most common prescriptions for rational problem-solving follow only if it is true.

Conventional descriptions of rational decision-making include the following steps: (i) clarification of the objective or values; (ii) survey of alternative means of reaching objectives; (iii) identification of consequences, including the side effects or by-products of each alternative means; and (iv) evaluation of each set of consequences in the light of the objective. However, Lindblom notes that such synoptic attempts at problem solving are not possible when, for example, clarification of objective founders on social conflict, when required information is not available or is available only at prohibitive costs, or when the problem is simply too complex for man's finite intellectual capacities. Most importantly, it does not logically follow, Lindblom argues, that when synoptic decision-making is extremely difficult it should nevertheless be pursued as far as possible. Hence he suggests that, in many circumstances, substantial departures from comprehensive understanding are not only inevitable but desirable. I cite his thesis in detail because the analogy to me seems so close.

Working Principles

I have summarized the case for what may be called "semi-planning." What are the working principles of this approach? A few major notions are worthy of consideration. (i) An element of laissez-faire, with its attendant duplication and gaps, may well be desirable rather than abominable. (ii) Orderliness, balance, and detailed planning may be more satisfying to the planners than to the society they serve; some matters probably ought to be left to what has been called "a wise and salutary neglect." (iii) It is unwise to specify detailed objectives in advance when the means of obtaining them are virtually unknown. (iv) A rational problem-solver wants what he can get and does not try to get what he wants except after identifying what he wants by examination of what he can get. (v) Arrangements must be established whereby decision-makers are made aware of, and can react promptly to, emerging problems. (vi) Long-range planning is a valuable exercise, but long-range plans for a research enterprise which is the sum of many smaller research programs are of dubious validity.

These principles, taken in part from Hirschman and Lindblom (*4*), approximate a real world which is almost invariably characterized by unbalanced, not balanced, growth. It is the above-scale salary offered to the new appointee which is the surest guarantee of an increase in the scale. It is the existence and success of the National Science Foundation which provides the platform on which stand those who argue for establishment of a National Humanities Foundation. Instances of the principle that imbalance results in pressure for a correcting growth are commonplace. And these same principles seem entirely germane to the planning of a national biomedical endeavor which is as inherently sporadic and random as is the natural growth of science itself. Indeed, the hallmark of the competent investigator is that he seeks constantly to identify the most important problem which can be attacked with the technology currently available and limits his goals accordingly. But his attention is continually given also to developments within his own and related disciplines. He is quick to apply new information, new techniques, new apparatus. In short, he brings to research his imagination, his knowledge, and his technical know-how, and he combines these with what may best be described as a "skillful opportunism."

In my view, those responsible for the management of a national enterprise which is the sum of such individuals must do likewise. They must continually assess the major parameters of the enterprise for which they have responsibility, continuing the attack on the major public-health problems, insuring the vitality of the classic scientific disciplines and recognizing the emergence of new ones, insuring the training of new investigators and practitioners, and safeguarding the health of the medical schools and universities. The total system may then be nourished and made to grow, but by disjointed increments. For example, given a 10- or 20-percent increase in total funds, one should almost never expand support, across the board, of all existing programs by this 10 or 20 percent. Instead, one should take advantage of significant, albeit unplanned and unexpected, new knowledge of human biology or pathology, of the work of new investigators as it appears, of new approaches, new drugs, new apparatus, new facilities, new architecture, and newly awakened public interest, always utilizing the skillful opportunism characteristic of the individual investigator.

Goals may be set only in the broadest terms of ultimate objectives—for example, a general homotransplantation, effective cancer chemotherapy, a rational management of viral infections, genetic transformation as therapy for hereditary disorders or the prevention of atherosclerosis. And one can, in a general way, plan *for* the tasks ahead by providing the necessary physical plant, stimulating activity in biomedical engineering, and providing a sufficient number of specialized facilities such as animal colonies, hyperbaric chambers, and libraries. It is highly doubtful that the planner can wisely do more; he will fail in his responsibilities if he does less. And he must ever be mindful that the planning of science must be left to the working scientist.

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Some Current Problems of Government Science Policy

What should be the balance between expenditures on pure and on applied science, and who should set it?

Harold Orlans

In the fall of 1963, much concern was evident in the scientific community about the course that several congressional committees would take in their inquiries into federal research and development programs; and the concern of interested parties is always evident at the time of the President's budget message and subsequent appropriations hearings in Congress. Now that the Select Committee on Government Research has completed its work and the House Subcommittee on Science, Research, and Development has finished its round of hearings and reports, I believe it would be generally conceded that the committee members and their staffs did an excellent and constructive job. Both committees—particularly the select committee chaired by Representative Carl Elliott of Alabama—broke new ground. It is not necessary to agree with every one of their recommendations to acknowledge that, under severe time pressure, they asked trenchant questions and gathered and published fresh and insightful information about the nation's gargantuan research-and-development enterprise. However, the fact that this special congressional effort was required to bring to light current and comprehensive statistics on such matters as the geographical distribution of federal R&D funds and the amount received by leading universities and companies suggests that the executive agencies responsible for informing the public about these expenditures had not been doing their job adequately. Let us hope that in the future these agencies maintain the standards of fuller and more timely reporting which have now been set with the assistance of Congress; for we can hardly expect to have either good current policies or adequate consideration of desirable new policies without comprehensive, timely, *and public* information about existing R&D programs.

As the rate of increase of federal R&D expenditures has been declining and as the volume of expenditures in major agencies like the Department of Defense, the National Aeronautics and Space Administration, and the Atomic Energy Commission has leveled off or declined, a major issue of public policy—and of public and private conflict within many agencies and their constituencies—has been posed: how much of the pie should go to basic research? Or, to put the matter another way, how much should go for research at universities, and how much for research and development in industry?

The Doctrine of the Sparrow

The answer of academic scientists is not entirely surprising: more should go to them. With a monotony that bespeaks a unison more than an originality of thought, they and their spokesmen in Washington argue that

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there is no danger of spending too much on basic research; that all "competent" university scientists should be supported to do work of their own choice; that science is an indivisible whole and all fields merit support equally (although some fields merit support more equally than others); that, while the results of any particular basic research project are unpredictable, it is not merely probable but virtually certain that the results of *all* basic research will yield a value greater than their cost. Some of the more ardent advocates of pure science even assert that the results of *any*—or *almost* any—pure research will *certainly* be rewarding, scientifically and socially, conceding only that one cannot predict precisely where or when the reward will be found or who will receive it. This may be termed a contemporary scientific version of the doctrine of the sparrow or the falling leaf—that no harm, no matter how slight, can befall a living thing without serving a higher moral purpose. As purposelessness and futility are thus vanquished in theology, if not in life, so, in the current eschatology of research, error, triviality, and important findings whose importance is unrecognized all equally serve the higher purposes of science. Thus, Alan Waterman, former director of the National Science Foundation, has declared that "The results of such [basic] research, in competent hands, are never without value. Even when no breakthroughs appear, the total effort always brings a possible breakthrough closer"; and he has spoken of "the statistical evidence [which was not, however, further identified] that most of the body of science ultimately achieves practical utility" (*1*). The fascinating justification of heavy federal expenditures on high-energy physics recently advanced by 30 distinguished physicists also dances delicately along the line of statistical likelihood—variously appraised as certain, probable, unlikely, and "not impossible" (*2*)—that these expenditures will yield a significant practical return. I do not doubt that they will yield *some* practical return: this one expects from the work of oafs, let alone that of brilliant men. The critical question—and I wish only to submit it, not to answer it—is: Will it yield a return commensurate with its cost, or greater than the return that can be anticipated from a comparable investment in other fields of science and technology (not to mention other areas of human endeavor)?

An Inconsistency

A striking inconsistency is apparent in the logic of many analyses of federal R&D policies. At times, the relation between the amount of money the federal government spends for basic research and the amount it spends for development is stressed, as when it is said that basic research expenditures are "only" x percent of the total R&D expenditures, whereas development expenditures are nx percent; therefore, it is argued, if economies are needed, the larger rather than the smaller amount should be cut, or carefully "scrutinized" (why not scrutinize both?). However, at other times, it is stressed that basic research should not be compared with development. Thus, it is, of late, increasingly contended that the basic-research expenditures of an agency should not compete with its expenditures on the development of new technology (which should compete instead with expenditures for the procurement and maintenance of existing technology, and other operating needs).

What, then, if anything, *should* basic research expenditures compete with? The answers to this question are frustratingly vague: indeed, no really satisfactory answer has yet been given, although there has been no lack of adventitious suggestions, ranging from expenditures on gambling or tobacco to some arbitrary percentage of the gross national product—all of which are proposed on the condition that they allow adequate scope for expansion. However, one significant suggestion was offered recently by the President's science adviser, Donald Hornig, in a letter to Senator Pastore, chairman of the Joint Committee on Atomic Energy, in which he stated that "the level and character of support for high energy physics must be determined and periodically reassessed in the context of . . . the overall national science program (rather than in relation to the applied research and development programs of the AEC) . . ." (*3*).

To my mind, there is still a good deal of usefulness in comparing expenditures on basic research with those on applied research and development, if only because these sums draw to one type of activity or the other men of comparable training—and I mean by this not only Ph.D.'s and Nobel prizewinners but the more numerous serfs of scientific and technical fiefdoms with mere master's and bachelor's degrees, and their auxiliary corps of glassblowers, machinists, secretaries, accountants, and groundkeepers. Although it was undoubtedly justifiable, immediately after the war, to complain that government expenditures on basic research were but a small percentage—evidently no more, and probably less, than 6 percent—of the $1 billion then spent on R&D, the 11 percent devoted to basic research in 1964 and the more than 14 percent proposed for 1966 out of some $14.6 billion (excluding capital plant and facilities) seem, on the face of it, not utterly, irredeemably, and tragically inadequate. Indeed, if the proportion of government funds going into university research were to be slightly reduced while that going into various forms of more direct aid to higher education were to be correspondingly increased, diverting a number of professors from laboratories to lecture rooms for another hour or two a week, the average quality of both education and research might well be enhanced.

If there is a portion of the R&D spectrum where national expenditures now appear patently inadequate to meet national and international needs, surely it is no longer the realms of pure science but those areas of obsolescent or inefficient civilian technology, at home and abroad, in which the prospect of private profit has been too dim to elicit enough private capital to ensure technical progress, while public expenditures have been blocked by the difficulty of devising a political formula acceptable to the major parties concerned. When the $7-million program of assistance for civilian technology proposed by the Department of Commerce a couple of years ago was rejected by Congress, it was noted in Sweden that their government was spending more—absolutely, not relatively—than the United States government on such programs. Surely, no R&D task merits greater priority today than the search for politically viable ways of utilizing engineers and scientists no longer required for military work to render industries like housing, transportation, textiles, and coal more efficient; to reduce the pollution of air, water, and soil; to improve our systems of education, medical care, and local government; and to raise the standards of living in impoverished areas of this and other nations. It is strange how much money and ingenuity are devoted to searching for indirect, accidental, and even surreptitious benefits of academic, military, and space research and development, and how little to R&D programs of direct and evident social and economic utility. Have we become so muscle-bound politically that, like Primo Carnera, we can display our strength but not use it where it is obviously needed?

A National Budget for Basic Research?

To return to Hornig's very interesting statement that the level of support for high-energy physics "must be determined . . . in the context of . . . *the* overall national science program . . ."—and the definite article which I have italicized is not the least interesting part of this statement, since it alludes to something which simply does not exist. What is advocated here for one field of science must, in principle, be applied to any and every other clearly recognized field. It appears, in short, that the president's science adviser is advocating the preparation of a national or at least a federal budget for all fields of basic scientific research. A number of other signs point in the same direction: the greater separation of government-wide expenditures on development and on research in Special Analysis H of the 1966 federal budget; the energetic and not entirely noncompetitive efforts by committees of the National Academy of Sciences to define and project desirable budgetary levels for various fields of science; and particularly the attempt by an *ad hoc* committee appointed by Academy President Frederick Seitz to grapple with two difficult but inescapable questions about scientific allocations posed by Representative Daddario's Subcommittee on Science, Research, and Development (*4*):

> 1. What level of Federal support is needed to maintain for the United States a position of leadership through basic research in the advancement of science and technology and their economic, cultural, and military applications?
>
> 2. What judgment can be reached on the balance of support now being given by the Federal Government to various fields of scientific endeavor, and on adjustments that should be considered, either within existing levels of overall support or under conditions of increased or decreased overall support?

Although some may feel that the Academy committee has dodged its assignment, rather than confronted it squarely, by answering these questions in the form of 15 separate essays written by individual committee members, it is nonetheless gratifying to see the questions being seriously faced at last, and the resultant document (*5*) is a significant contribution to the thin but growing literature on the aggravating problem of scientific choice.

The new efforts of the National Science Foundation to examine the same problem of scientific allocations should also be noted; these were reported recently by Foundation Director Leland Haworth (*6*):

> The Foundation . . . plans to give additional emphasis to the compilation and analysis of data which bear specifically on the question of relative total levels of support and measures of apparent needs. . . . Thus, we hope eventually to be able to cite fairly precise figures relative to the average amount of total research support available to academic scientists, by field of science, and to augment such data with judgments from competent people in the various fields on the question of reasonable ranges of support levels for each discipline. . . . The problem of making interfield priority judgments should become more manageable if somewhat more complete information on a field-by-field basis can be made available.

The establishment of such a central data bank on federal research grants and contracts is to be commended and should materially assist the rational allocation of scientific expenditures by both public and private agencies—provided that the raw data are not husbanded and used by one camp or another as a weapon in its struggle for a share of limited funds, but are made freely available to all to enlarge our knowledge of national allocations to—and returns from—various fields of science. Too often in the past certain data relevant to public policies have been regarded as proprietary and released only in politically convenient tabulations. Let us, again, hope that the agencies responsible for formulating federal policies for science will adopt the same principle of the fullest possible disclosure of data that is universally accepted with regard to the data of science itself.

No one observing the Washington scene can, however, be so deluded as to believe that key decisions always are or can be made in public and based solely on considerations known to the public. The inner councils of government are always, to some extent, obscure; the passage of time gradually enlarges the public record of private deliberations while reducing both its authenticity and its relevance to future decisions; and available records of the process of decision in major scientific programs are sparse indeed. Except for such information and insight as can be gleaned from congressional hearings, evidence is not generally taken in public; deliberations proceed behind the necessary or convenient cloak of executive or legislative privilege, and the final pronouncement commonly resembles a brief for one side more than a dispassionate examination of available alternatives.

The Composition of the Jury

In this situation where verdicts are reached in private, the composition of the jury assumes a special importance: it provides, in fact, the principal visible assurance that justice is being done. The composition and method of selection of important scientific policy groups therefore merits continuing public scrutiny and discussion. Social scientists have managed to secure representation on an enlarged section of the National Research Council and an occasional appointment to the National Science Board, but none has yet been selected for the President's Science Advisory Committee. Engineers have been so dissatisfied with their status in the National Academy of Sciences that they have formed an academy of their own. The composition of the President's Science Advisory Committee was perhaps adequate to its earlier responsibilities of advising upon the value of proposed weapons systems. However, as the committee's responsibilities have broadened to the formulation of general government policies for science and technology, and as the machinery for implementing its advice has been strengthened, the committee's credentials for performing these larger tasks should be periodically reassessed. The geographic concentration of its members has fortunately been broadened by the latest round of appointments, but the addition of a few more members from industry and a few from selected fields of social science would strengthen the committee's competence to deal with some of the problems which it now faces.

Finally, a few words about what is sometimes regarded as the missing link in the establishment of national policies for science and technology: Congress. Congress has been quicker to see, and to act upon, deficiencies in the executive's formulation and coordination of R&D policies than to remedy its own deficiencies. There is a clear need for improved mechanisms within Congress, comparable to those which have been developed in recent years within the executive, for handling the flow of scientific programs and budgets on a basis that is broadly consistent and campatible with the national interest. The appointment of a new unit in the Legislative Reference Service of the Library of Congress to provide information on scientific and technological programs and policies, the continuing work of the Daddario subcommittee, and the establishment of the new permanent Subcommittee on Research and Technical Programs of the House Committee on Government Operations, under the chairmanship of Representative Henry Reuss of Wisconsin, indicate a recognition of the problem. Is it too sanguine to foresee further Congressional steps to define national rather than sectional goals for science and technology and to enlarge the authority of Congress as a whole in the making of science policies?

Summary

The problems of government science policy I have noted are not exactly new, but each has, I believe, acquired a new degree of urgency from the pressure of events: How much should be spent on basic research and how much on civilian technology? How can reasonable allocations be made among various fields of science? Who is to make these allocations, in the executive and in Congress? The degree to which we can, by objective research and perceptive analysis, accommodate the accidents of history and politics to the changing needs of science, industry, and society will determine the degree to which we can serve not the interests of those groups and individuals (both scientists and politicians) who happen to be in positions of power, but the present needs of the nation.

References and Notes

1. A. Waterman, Director's Statement, in *Nat. Sci. Found. Ann. Rept.* **8**, xii (1958); *Science* **147**, 16 (1965). In contrast, John Jewkes argues that "the overwhelming mass of scientific thought and observation in the Western world was never designed to bring, has not brought, and is highly unlikely ever to bring the slightest improvement in material standards of living" [*Economic Journal* **70**, 14 (Mar. 1960)].
2. See R. Sachs, H. Primakoff, H. A. Bethe, and J. R. Oppenheimer, all in *The Nature of Matter: Purposes of High Energy Physics*, L. C. L. Yuan, Ed. (Brookhaven National Laboratory, Upton, N.Y., Jan. 1965); see also *Science* **147**, 1548 (1965).
3. From letter, dated 27 March 1964, from Donald Hornig to Senator John Pastore, in *High Energy Physics Program* (Joint Committee on Atomic Energy, Feb. 1965), p. 57.
4. *Basic Research and National Goals* (Committee on Science and Astronautics, House of Representatives, Mar. 1965), p. 1.
5. *Basic Research and National Goals*, report of the Committee on Science and Public Policy, National Academy of Sciences (U.S. Government Printing Office, Washington, D.C., April 1965).
6. L. Haworth, "Director's statement," in *Nat. Sci. Found. Ann. Rept.* **14**, xxv (1964).

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Do We Need a Department of Science and Technology?

A recurring issue gets a fresh appraisal and a positive answer.

Herbert Roback

Departmental status for science in government is not a novel idea. It was broached 85 years ago by a committee of the National Academy of Sciences reporting on the organization of government science bureaus. The committee was appointed in 1884 at the behest of a member of the National Academy, Theodore Lyman, who was also by unique coincidence a member of Congress. The scientist-congressman, whose National Academy standing was gained by his researches on the Ophiurida, was instrumental in placing a rider on a sundry civil appropriation bill which set up a joint congressional committee (called a commission) to study the organization of the government science agencies (*1*). This was a compromise measure, Lyman told his House colleagues in urging its acceptance, his main concern being to save the Coast and Geodetic Survey from takeover by the Navy (*2*). Apparent duplication between the Coast Survey and the Navy's Hydrographic Office in charting coastal waters had led the Navy to espouse merger, a proposal which had considerable appeal in the 48th Congress. The legislative rider directed an investigation of the activities and interrelationships of the Coast and Geodetic Survey, the Hydrographic Office of the Navy, the Geological Survey, and the Signal (Weather) Service. The National Academy committee, enlisted by Lyman, gave technical support to the congressional commission.

Men of science were leery then, as they are now, of military dominance in scientific enterprises. Many of them argued that science agencies should be taken from, rather than placed in, the military departments. For example, they wanted the Naval Observatory to be a *national* observatory, and the weather service to be removed from the Army Signal Corps. At the same time, they recognized that better coordination of the government's scientific work was needed, and various proposals were made toward that end. The aforementioned report of the National Academy committee crystallized the issues. This group was convinced that the science agencies should be pulled together "under one central authority," but the particular form of organization they left to the future and to Congress. Then the committee ventured this cautious but significant observation (*3*):

> . . . The best form would be, perhaps, the establishment of a Department of Science, the head of which should be an administrator familiar with scientific affairs, but not necessarily an investigator in any specific branch. Your committee states only the general sentiment and wish of men of science, when it says that its members believe the time is near when the country will demand the institution of a branch of the executive Government devoted especially to the direction and control of all the purely scientific work of the Government.

The NAS committee went on to say that, if public opinion was not yet ready to accept a Department of Science, the next best step would be to move the several scientific bureaus into one of the existing departments. Even then coordination would not be automatically insured, in the committee's view, and so they recommended the "organization of a permanent commission to prescribe the general policy for each of these bureaus." The commission would "examine, improve, and approve" plans of work and expenditures and recommend efficiency measures but abstain from administrative involvement. This would be a nine-member commission composed of scientists drawn from government and private life (*4*).

End of an Idea

The congressional commission, reporting in 1886, gave short shrift to the suggestions both for a Department of Science and a supervisory commission. A new department was held not justified by the degree of duplication in existing scientific agencies; a coordinating policy group was deemed impracticable because department heads could not very well relinquish to subordinates and outsiders their responsibilities for general direction and control (*5*). With this dismissal by an agency of the Congress, the Department of Science idea died aborning, though it was actively debated at the time in scientific circles (*6*). In the ensuing decades not much was heard about it. Proposals for government departments were made in the fields of health, education, labor, industry, commerce, and agriculture, separately or in various combinations, and three cabinet departments (Agriculture, Commerce, Labor) were established between 1885 and 1945. Not until 1946 was the Department of Science idea revived, at least in the legislative halls. Clare Booth Luce, then a Representative from Connecticut, introduced a bill (H.R. 5332, 79th Congress) to create a Department of Science and Research stressing the need for national self-preservation in the atomic age and the importance of attracting young people to science careers. Mrs. Luce said: "Only the prestige which attaches to a regular member of the cabinet will render the findings of any scientific body of sufficient weight to command the constant attention of the highest officials of the Government in the consideration and formulation of policy" (*7*). The bill was pigeonholed by a House committee.

Vannevar Bush was working for the establishment of an independent agency, which he called the National Research Foundation, to sponsor research of military as well as civilian interest (*8*). He proposed that it be governed by a director and part-time board of nongovernment scientists. A separate group of nongovernment scientists, which he called a Science Advisory Board, would coordinate the work of government science agencies. These pro-

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posals, outlined in Bush's 1945 report to President Truman, "Science—the Endless Frontier," were modified in legislative measures to become a National Science Foundation. The bill which the Congress passed was vetoed in July 1947 by President Truman, who objected to control of government science policy by an outside board (*9*). The criticisms were reminiscent, in some respects, of those heard in 1884–5 to the National Academy committee's proposal for a science policy commission which would include outside as well as government scientists.

The Bush report was followed in 1947 by the Steelman report, "Science and Public Policy," which went over much of the same ground but with closer orientation to the routines of governmental administration. The Steelman report called for a National Science Foundation to be organized "on sound lines" and suggested that the agency be located in the Executive Office of the President until other federal programs in support of higher education were established, after which time consideration could be given to grouping all such activities, including the National Science Foundation, in a single agency. The Steelman report also favored a part-time governing board for the NSF, but government as well as outside scientists were to be included. It also recommended the creation of an interdepartmental committee on scientific research and development, a special unit in the Bureau of the Budget to review government science programs, and a member of the White House staff to be designated by the President for purposes of scientific liaison (*10*). The Steelman report eschewed any radical departure from the existing framework, presumably meaning that a Department of Science was not in the cards. Three years elapsed, however, before the differences in the several approaches to a National Science Foundation were compromised and a bill finally enacted into law (*11*).

Legislative Renewal

Sputnik generated a new debate on departmental status for science in the Congress led by Senator Hubert H. Humphrey. On 27 January 1958 a broad-based bill, S. 3126, was jointly introduced by Senators Humphrey, McClellan, and Yarborough to create a Department of Science and Technology which would coordinate and improve federal functions relating to the gathering, retrieval, and dissemination of scientific information; provide educational loans to students in certain science fields; establish national institutes of scientific research; and establish cooperative programs abroad for collecting, translating, and distributing scientific and technological information. A day later Senator Kefauver introduced S. 3180 to create a Department of Science. Both bills were referred to the Committee on Government Operations.

Jurisdictional questions were raised, presumably because the bills went beyond organizational matters into policy, and at the request of Senator Lyndon B. Johnson, they were referred anew to his Senate Special Committee on Space and Astronautics, which had been created to consider the government's response to the Russian triumph in space. Without these bills, the Committee on Government Operations was unable to hold hearings in the 85th Congress on the proposal to establish a Department of Science and Technology, which was incorporated in Title I of the bill, but it directed its committee staff to maintain a continuing study of that area. The Humphrey subcommittee did manage, after an agreement reached with Senator Johnson, to hold some hearings in May–June 1958 on a limited aspect of Title I, the proposal for a scientific information center (*12*).

To narrow the jurisdictional issue and regain control of the organizational aspect, the sponsors of the Humphrey bill, now reinforced by Senators Ervin, Gruening, and Muskie, split off Title I and introduced it, with certain revisions, as S. 676 in the 86th Congress. It proposed a transfer to the new department of the National Science Foundation, Atomic Energy Commission, National Aeronautics and Space Administration, National Bureau of Standards, and certain activities of the Smithsonian Institution. By then the impetus for a new department was considerably diminished by NASA's presence. The thrust of science organization was less to coordinate and align than to reach out and do, for Sputnik had caused hurt pride and fear in the nation. It was difficult to make a case for legislating a new department to absorb NASA when the ink was hardly dry on the President's signature to the National Aeronautics and Space Act (*13*).

Indeed, the rush of legislative events and the flurry of organizational activity in the executive branch during 1958 outpaced the committee's deliberations on the suitable form of a bill. The Congress created along with NASA an Aeronautics and Space Council and a standing committee in each house to monitor space and related activities. The Defense Education Act gave support to science education and facilities. A reorganization act for the Department of Defense established a Directorate for Defense Research and Engineering. The Advanced Research Projects Agency, previously established as the military's own response to Sputnik, was made an adjunct of the new directorate. The President acquired a Special Assistant for Science and Technology and gave White House status to the Science Advisory Committee. The Federal Council for Science and Technology replaced a looser interdepartmental committee of similar function. Science advisers were assigned to both the Secretary of State and the Secretary General of NATO. A NATO science committee signified the outward reach of science for defense, while "Atoms for Peace" and the International Geophysical Year represented a peaceful gesture to a world community of science. "Altogether," as James R. Killian, Jr., said before the AAAS in summing up government science for 1958, "the year brought an impressive array of organizational innovations for the management of government programs in science and technology and for the provision of scientific advice at policy-making levels" (*14*).

Executive Opposition

The spokesmen for science at the Presidential level made plain their distaste for a Department of Science and Technology. Killian, speaking at the AAAS meeting as the President's Assistant for Science and Technology, took pains to quote from Don K. Price's 1954 study: "In the organization of the Government for the support of science we do not need to put all of science into a single agency; on the contrary, we need to see that it is infused into the program of every department and every bureau" (*15*). The President's Science Advisory Committee in its new eminence regarded a Federal Council for Science and Technology as the instrument for achieving coordination and cooperation among government science agencies. A single department, in PSAC's collective view, would not be able satisfactorily to administer either the mission-oriented scientific and technical functions of existing departments or the "unique" specialized programs of AEC, NASA, and NSF. This seemed to be the prevailing sentiment among scientists, though there were notable exceptions. Lloyd V. Berkner would settle for a department excluding the three aforementioned independent agencies; Wallace R. Brode would combine them with a host of others, including the National Institutes of Health, in a Department of Science and Technology (*16*).

Perhaps the strongest argument from a practical standpoint against immediate legislative action—that the President had not recommended a new department—was made by Representative John W. McCormack as chairman of the House Select Committee on Astronautics and Space Exploration. He wrote to Senator Humphrey in 1958 (*17*):

> While I believe there should be a Department of Science, I feel that until whoever is President either recommends the establishment of such a Department, or would not object to such a Department being established, it would be unwise to force such a Department upon them. I want you to know that I am strongly in favor of a Department of Science being established and, in my opinion, it is only a matter of time that one will be established.

In March 1959 in a review of the state of science affairs, the Humphrey subcommittee observed morosely (*17*, p. 19): ". . . there have been certain administrative actions taken which tend to evade the question as to whether a Department of Science and Technology is necessary or desirable, and there are a number of indications from the scientific community that there will be opposition to such a proposal, at least until the need therefor has been more clearly established."

The subcommittee held hearings in April 1959 on S. 676 and S. 586 (Senator Kefauver's bill) to establish a Department of Science. Senator Humphrey, aware of the opposition, hedged a bit. His opening statement said that the proposed Department of Science and Technology was to be considered one possible solution to the problems of centralization and coordination of federal science programs and operations, but not a final conclusion of the committee. The witnesses before the subcommittee were divided. Lewis L. Strauss, as Secretary of Commerce, opposed departmental status for science. Brode, as scientific adviser to the State Department and chairman of the AAAS, strongly favored it. Others pressed for a stronger advisory apparatus at the Presidential level or a study to determine the need for a department and what agencies should be included (*18*). It was easier to agree on a study commission which, to the advocates of a department, appeared better than nothing, to the dubious, a means of seeking more information, and to the opponents, a device for deflecting action on a controversial subject.

At the conclusion of the April 1959 hearings, the staff of the Senate Committee on Government Operations drafted a bill proposing the establishment of a Commission on a Department of Science and Technology. This was introduced in the Senate on 5 May 1959 as S. 1851, under the joint sponsorship of Senators Humphrey, Capehart, Mundt, Gruening, Muskie, Yarborough, and Keating. In a 1-day hearing (28 May) on S. 1851, S. 676, and S. 586, the subcommittee heard no comforting words from the Eisenhower Administration. Alan S. Waterman, whose NSF budget had been increased from $50 million to $136 million after Sputnik, opposed both a Department of Science and Technology and a commission to study the matter. The Bureau of Budget representative, the official spokesman on all matters dealing with reorganization, did likewise, doubting that "the scientific members of the Commission would necessarily be best able to judge the optimum form of Government organization in this field." Leonard Carmichael, secretary of the Smithsonian Institution, endorsed the study commission but suggested that, if it were established, the membership nominations be made by the National Academy of Sciences (*19*).

Notwithstanding the administration's opposition, Senator Humphrey for the Committee on Government Operations reported S. 1851 favorably on 18 June 1959 (*19*). A bipartisan commission was needed, the report said, so that "the Congress and the President may have the benefit of the recommendations of qualified experts in the fields of science, engineering, and technology" as the basis for legislation to improve federal science programs and operations. The committee justified a study commission mainly on the ground that the Congress needed more and better information. As a case in point, Killian had politely declined an earlier invitation to appear before the committee because it might conflict with his advisory role in the White House. Science policy coordination or control at that level, in the committee's belief, would not assure an ample flow of scientific and factual data to the Congress. The Department of Science and Technology, or at least a commission to study its feasibility, was the committee's proposed solution. The Senate did not take up the bill. A companion House bill (H.R. 8325) introduced on 22 July 1959 by Representative Brooks of Louisiana, chairman of the Committee on Science and Astronautics, was referred to the Committee on Government Operations but received no action.

The OST Alternative

Early in 1960 Senator Humphrey put the case for a department or a commission before the American Academy of Political and Social Science (*20*). But those who favored strengthening the Presidential advisory apparatus rather than a new department for science found a champion in another subcommittee of the same Senate committee—that on National Policy Machinery chaired by Senator Henry M. Jackson. The Jackson subcommittee held hearings in April 1960 on the role of science and technology in foreign and national defense policy. A staff report of 14 June 1961 entitled "Science Organization and the President's Office" rejected the Department of Science idea on the by now familiar ground that the diverse scientific activities of the federal government could not be conveniently extracted to form a new department. It approved such views expressed before the subcommittee by James Fisk, president of Bell Telephone Laboratories, and then observed (*21*):

> Eight departments and agencies support major technical programs and all parts of the Government use science in varying degrees to help meet the agency objective. This diffusion of science and technology throughout the Government is not a sign of untidy administrative housekeeping. Rather it reflects the very nature of science itself. Organizationally, science is not a definable jurisdiction. Like economics, it is a tool. It is an instrument for accomplishing things having nothing to do with science.

The staff report emphasized the President's responsibility for science policy direction and accordingly recommended the strengthening of his advisory support by the creation of an Office of Science and Technology. It pointed out that the President could take this step through submission of a reorganization plan rather than through the conventional legislative route. The Kennedy administration was asked to submit to the Congress by January 1962 "its considered findings and recommendations for action." On 29 March Reorganization Plan No. 2 of 1962 creating the OST was submitted, to take effect within 60 days if the Congress did not disapprove (*22*).

Before the plan was formally sent to the 87th Congress, S. 2771 was introduced on 31 January 1962, jointly sponsored by Senators McClellan, Humphrey, Mundt, Cotton, and Yarborough. S. 2771 was similar to S. 1851 of the 86th Congress, which had been reported favorably by the Senate Committee on Government Operations. The revised bill contained a broad declaration of congressional policy and objectives in science and placed more emphasis on the need for improvement in federal programs for processing the retrieval of scientific information. It also provided that the 12-member commission be strengthened by a scientific advisory panel with prescribed qualifications which included "ability to communicate not only to professional scientists but to laymen." Hearings were held on 10 May and 24 July 1962. Some moral support was provided by Carl F. Stover's report of March 1962 on "The Government of Science" to the Center for the Study of Democratic Institutions. A Department of Science and Technology, the Stover report said, would establish for science a major center of policy studies, higher stature, and a more favorable environment for scientific work. Combining all government science functions made no sense, but a single department for those functions less mission-oriented was "a sound and desirable next step in the evolution of Government action with respect to science" (*23*).

The committee now had to take judicial notice of the alternative scheme recommended by the Jackson subcommittee and seized upon by the Kennedy Administration as a sufficient response to the demands for improved science organization. Administration spokesmen pointed to OST as a needed mechanism for coordinating science policies and advising the President, whatever the organization of science functions for the government as a whole. Waterman, who was assessing NSF's truncated policy role in the wake of the OST plan, again opposed a commission, as did Elmer B. Staats, deputy director of the Budget Bureau, where all reorganization plans are put together. Their plea was that OST, being new, should have a chance to work. Furthermore, by the "statutory underpinning" of a reorganization plan, OST would give the Congress the kind of access to scientific information sought by the sponsors of S. 2771. This was the persuasive point for congressional acceptance of the plan (*24*).

Jerome B. Wiesner, who would serve the Kennedy Administration in the quadruple capacity of OST director, President's science adviser, chairman of the President's Science Advisory Committee, and chairman of the Federal Council for Science and Technology, made his first appearance before Congress as OST director when he testified on 31 July 1962 at hearings of the Holifield subcommittee (House Committee on Government Operations). In amplifying his views on science organization, Wiesner gave conditional endorsement to a Department of Science. To "set up a radically new organization" encompassing all the scientific activities of the federal government he considered unworkable. If a "less comprehensive Department of Science were created," including the Atomic Energy Commission, National Science Foundation, National Bureau of Standards, and certain other agencies, he believed the operations of these agencies might be improved. At the same time, the need would remain to coordinate and integrate the activities of these agencies with the related scientific and technical programs of the mission-oriented agencies. "In other words, the OST is neither a substitute for nor in competition with a Federal Department of Science" (*25*).

The Senate Committee on Government Operations, not daunted by the new presence of OST, reported favorably (with some technical revisions) on S. 2771, proposing a Commission on Science and Technology (*26*). The bill passed the Senate by unanimous consent on 8 August 1962 (*27*). In the House it was referred to the Committee on Science and Astronautics on 9 August, and there it died. The exercise was repeated in the 88th Congress. S. 816, sponsored by Senators McClellan, Humphrey, Mundt, Gruening, Javits, Cotton, and Yarborough, was introduced on 18 February 1963. Chairman McClellan, now the leading sponsor, emphasized that Wiesner, in his testimony before the Holifield subcommittee, maintained that OST and a Department of Science and Technology were not in conflict (*28*). The bill was approved by the Senate Committee on Government Operations and reported to the Senate on 4 March 1963 (*29*). It passed the Senate by unanimous consent on 8 March (*30*) and was referred to the House Committee on Science and Astronautics, which also had a companion bill, H.R. 4346, introduced by Representative Teague of Texas (*31*). No action was taken on these bills in the House committee.

In place of a mixed commission, the reaction on the House side was to create several new subcommittees on science. Thus in August 1963, the House Committee on Science and Astronautics created a Subcommittee on Science, Research and Development, chaired by Representative Daddario of Connecticut. And the House of Representatives, a month later, created the Select Committee on Government Research, chaired by Representative Elliott of Alabama. The Select Committee took a dim view of departmental status for science, judging by its tenth and concluding report of 29 December 1964, which contained this statement (*32*):

> The specters of overlap, gaps, conflict, and duplication among agency programs can best be met through adequate top-level coordination of agency programs. Consolidating research and development into one or a few separate agencies—such as an often suggested Department of Science and Technology—would separate such work from the purposes for which it is performed, the committee believes, with devastating effects both to the work and to the capacities of agencies to carry out their missions.

In the 89th Congress Chairman McClellan, joined by Senators Mundt, Ribicoff, Gruening, and Yarborough, reintroduced the commission bill (S. 1136) on 17 February 1965, and Representative Wolff sponsored the companion bill (H.R. 5609) in the House. By now congressional interest in the proposal had waned. No hearings were held, and the Senate committee did not bother to report it out. Humphrey, no longer a Senator, presided over the Senate as Vice President and became immersed in intricacies of space and ocean programs as statutory chairman of technical councils in these areas. Occasionally, other voices renewed the call for a department. Ralph Lapp proposed a Department of Science in his 1965 book, *The New Priesthood* (*33*). J. Herbert Holloman, after 5 years as Assistant Secretary of Commerce for Science and Technology, recommended to the Ribicoff subcommittee in 1968 that a Department of Science and Technology be a prime subject for study by a proposed Commission on Organization and Management of the Executive Branch (*34*). At the year's end, Donald F. Hornig, from the vantage point of "five years at the bench of U.S. science policy," spoke out before the AAAS in favor of a Department of Science as well as a strengthening of the President's science advisory setup (*35*).

As one traces the lines of argument for and against departmental status for science, it is apparent that they thread back to the controversy of the 1880's. The positive side, projected by the NAS committee report of 1884, is that science will benefit from the status and prestige which go with cabinet rank and large departmental resources. The negative side, well stated by Secretary of the Navy William E. Chandler before the congressional commission in 1884, is that science is not a government mission in itself but an aspect of other and proper departmental missions; consequently science bureaus or functions should be placed or remain within the department to which they are "naturally related" (*36*). Contemporary formulations haven't improved much on these themes. Proponents of a separate department for science view its secretary as a protector and spokesman of science in government councils, while opponents see a bureaucratic monstrosity in which politics prevail over scientific objectivity. On both sides attitudes are hardened by conviction or softened by practical considerations. Doubtless many who are otherwise well-intentioned toward a new department fear that it would cut down opportunities for grants and contracts given by various uncoordinated government science agencies. Others who are moved more by a concern for economy in government than for prestige in science believe that departmental organization would eliminate duplication and insure closer coordination of costly government programs.

Case for a Department

That it is impracticable to tear out research and development functions from department and agency settings and bring them all together in a new department goes without saying. But the case for a Department of Science and Technology cannot be that easily dismissed. To argue that science is a means and not an end, or that science (and technology) is not by itself a major purpose of government justifying departmental organization, narrows the issue unduly and overlooks some very practical problems. Agricultural research, let us quickly agree, is properly a part of the Department of Agriculture mission, but what about the large relatively self-contained or semiautonomous agencies with missions which fall almost completely in the domain of science and technology and which overshadow in size and importance some of the older departments? If AEC's mission is atomic energy development and NASA's is space exploration, it is merely tautological to distinguish these missions from science and technology in given fields. Then it becomes a pragmatic problem of government organization (and politics) to determine whether it is advantageous to bring together in a single department selected agencies and subagencies associated by shared purposes, related functions, or some other defining element of mutual involvement. Modern precepts of government organization and administration favor a relatively few strong departments encompassing similar or related functions in place of a profusion of independent agencies. The quest here is more compelling than a desire for organizational symmetry or housekeeping tidiness. The President, as manager of the executive branch, does not have the time to deal with scores of agencies. To maintain a proper "span of control" he must strive to bring these agencies within departmental confines and depend on the department heads to administer the manifold affairs of government (*37*).

The challenge is that government in all its diversity does not lend itself easily to departmentalizing by major purpose or mission or any other organizing principle. Most organizational arrangements are less ambitious—expedient responses to urgent problems dictated more by politics than political science. Government takes on a patchwork appearance. From time to time attempts are made to sort out and rearrange agencies and functions in more orderly patterns, even to the extent of disestablishing or reforming old departments. Not every worthy government cause which seeks wider acceptance and ampler resources through separate departmental status can be accommodated. A multiplicity of departments would defeat the rationale for departmental organization. On the other hand, if a department embraces too many missions or disparate functions, it becomes unwieldy—a conglomerate or a holding company in which the secretary struggles constantly to keep in line strong-willed administrators of operating agencies.

In a dynamic, democratic society, governmental reorganization, despite the obstacles, signifies changing policy, a new approach—and reorganization on a departmental scale makes the greatest impact. Accordingly every administration can be expected to give special attention to such possibilities. Since World War II, each President has opted for a new department—Truman for DOD, Eisenhower for HEW, Kennedy for HUD, and Johnson for DOT (*38*). The Nixon Administration has established an advisory group on reorganization, whose recommendations are yet to be made (*39*). Characteristically, the post-World War II departments each represent a coalescence of established agencies and resources to subserve a broader policy or purpose of government. In several instances, the way was prepared by interim coordinating organizations. Thus, the DOD was preceded by a looser federation formally known as the Military Establishment, HEW by the Federal Security Agency, and HUD by the Housing and Home Finance Agency. The Department of Transportation, the latest departmental creation, did not go through a transitional form but established transportation agencies were a base upon which to build.

Science and technology, comprising large sectors of government activity with various organizational forms, have a similar potential for departmental organization. When great national problems arose, requiring positive and pointed government response, independent agencies were created—the AEC for the control of atomic energy after Hiroshima, the NSF to preserve the post-World War II momentum of research and development, and NASA after Sputnik. With the passing years, as missions are completed or redirected and as agencies mature, it is difficult to maintain the momentum and the excitement of the early days. New problems emerge, priorities are reassessed, talents are turned elsewhere. The atomic energy program is about 25 years old, the NSF has been in business 18 years, and the space agency, past its 10th birthday, will age rather quickly after a lunar landing. Reorganization generates its own excitement, infuses new energies, develops new missions.

Candidates for Inclusion

Thus AEC and NASA, independent technical agencies with multibillion-dollar yearly budgets, are prime candidates for transfer to a new department. Their interests increasingly will overlap as boosters and spacecraft come to depend more on nuclear technology. Both are sponsors of hardware development as well as basic research. Both are involved in intricate ways with Department of Defense programs. Both have large laboratory complexes and diversified resources for research and development. Both are faced with probable cutbacks and the need to reassess missions for the long term. The reassessment, in NASA's case, is associated with the moon landing, which will climax a decade of technical effort directed largely to this single goal. New vistas of space exploration beckon, but in the welfare decade of the 1970's more earth-bound causes will exert a strong gravitational pull on funds.

As for the AEC, the growth of nuclear stockpiles to what many regard as overkill dimensions and the gradual shift to industry of responsibility for nuclear power development are less climactic. The safety and regulatory functions associated with nuclear power, which some foresee as AEC's major responsibility ahead, could well be transferred to the Federal Power Commission, possibly helping to rejuvenate an old-line agency, just as the Federal Communications Commission has had to grapple with the regulatory aspects of satellite communications. Nuclear ordnance development and fabrication possibly could be shifted to the Department of Defense (*40*). The Department of Science and Technology would have, one may conceive, a space service and an atomic service, perhaps less ambitious than at present but still performing vital scientific and technical work. The reorganization also would permit a realignment and better integration of the great laboratory complexes associated with these two agencies. Indeed, the realigning process for federal laboratories as a whole could be speeded up by this means.

The National Science Foundation is a somewhat different type of agency. It maintains no laboratories except a few contract research centers and builds no large projects or systems, with the exception of the ill-fated Mohole project. It values its relative independence and freedom from political influences in supporting academic science. In terms of prospective departmental status, it could be argued that NSF has as much affinity with education as with science, and if a separate Department of Education were to be created, undoubtedly there would be advocates for inclusion of NSF. On the other hand, education reaches out toward areas of contemporary concern not closely identified with science, such as job training and placement and manpower development, so that some envisage education as the organizing principle for a Department of Human Resources (*41*). Hornig favors the science-education nexus. He would make NSF the "core" of a Department of Science, linking basic research closely with higher education. In this concept, the new department would be little concerned with technology as distinguished from science, leaving technological development to "agencies with specific tasks and missions" (*35*).

In the writer's view, the prospects for departmental status are greatly improved if technology and science are conjoined. Creating a new department is difficult enough in itself, but technology provides more leverage and power for organizational change than basic research or pure science. The new department would need a bigger core or a broader base than that offered by NSF alone. In any event, the writer sees no serious obstacle to making the NSF a component of a Department of Science and Technology. In that way grants and other financial support to academic institutions could be better integrated, since NASA and AEC also are substantial contributors to academic science. Furthermore, the 1968 amendments to the National Science Foundation Act add applied research to the agency's reponsibilities and thereby bring it closer to the technological concerns of other government agencies (*42*).

There is good logic in establishing a Department of Science and Technology to house not only older, more mature agencies but also new ones which have not yet found a suitable home. Oceanography and related disciplines or technologies may be put in this class. Numerous government agencies are engaged in marine science activities, but the Congress has been groping for a decade or more to find the organizational base for a broad program of ocean development. The 1966 legislation, which created a temporary commission and a council for marine sciences and resources, stated a policy and provided a coordinating group but sidestepped the basic organizational problem (*43*). The Commission on Marine Sciences, Engineering and Resources, on the eve of its demise, proposed that a National Oceanic and Atmospheric Agency be created as "the principal instrumentality within the Federal Government for administration of the Nation's civil marine and atmospheric programs." At the same time, the commission pointed out that it was proposing "an organization which can easily fit into a more fundamental restructuring of the Federal Government" (*44*). Clearly, the commission was leaving the door open for incorporation of marine sciences and resources in a Department of Science and Technology

Immediate Advantages

One of the immediate advantages in creating a new government house for science and technology is the opportunity it affords for eliminating the clutter in the Executive Office of the President or at least making room for needed new services. The Aeronautics and Space Council and the National Council on Marine Resources and Engineering Development both could be abolished or, along with PSAC and OST, shifted in whole or in part to the new department, though it must be recognized that the President will continue to need a science adviser with some staff of his own. The Vice President, now statutory chairman of the space and marine councils, could retain his valuable association with government science and continue to gain the technical information and insight needed for leadership in our technocratic society by serving in some appropriate capacity, possibly as chairman of the advisory apparatus annexed to the new department. The Office of Telecommunications Management, for want of a better alternative, also could be housed in the Department of Science and Technology. This office needs strengthening to deal with communications problems of growing severity and technical sophistication. The Post Office and Transportation Departments each could make a claim for telecommunications management, but obviously they have enough problems of their own.

The removal from the Executive Office of its scientific or technical councils and offices is not a downgrading of science but a practical recognition that the President cannot give them sustained attention (*45*). Moreover, they have less impact on affairs than is usually supposed. Their directors parade before the government departments and agencies clothed in the uniform of Presidential prestige but are uncertain to what extent they can speak or act in his name. The department head directing a broad range of scientific and technical programs with a large budget has power and prestige of a more compelling kind. His command of resources, public visibility, and cabinet participation enable him to serve as principal science adviser to the President in a much more direct and positive way than the White House adviser or Executive Office functionary several steps removed from the scene of departmental action and operations. If the scientific community is concerned about prestige for science in government, there is considerable trade-off value in a department head as against the Executive Office coordinator or consultant.

Another advantage is that the new department could house technical agencies or bureaus which are obstacles to, or casualties of, other reorganizations. For example, in January 1967, President Johnson proposed a merger of the Departments of Commerce and Labor (*46*). He did not push the proposal when the response in congressional and some other quarters seemed unfavorable. Despite the inevitable resistance, there was merit in a merger, the objective being a department of economic affairs or economic development. Since the Department of Commerce has acquired by historical accretion a number of important technical services now encompassed in the Environmental Science Services Administration, the National Bureau of Standards, the U.S. Patent Office, and other units, it would have made sense, in the event of a Commerce-Labor merger, to extract these technical agencies and place them in a Department of Science and Technology.

Finally, a Department of Science and Technology would provide better interface with the Department of Defense. Although it would not be wise to transfer research and development commands, offices, or agencies from the Department of Defense to the civilian department in any wholesale fashion, conceivably several military-managed laboratories, agencies, or programs could be transferred on a selective basis if their relationship to military needs is limited, if they now serve many government users, and if their concern is more with science than with defense (*47*). A civil department conveniently could assume DOD responsibilities in supporting educational centers of excellence or sponsoring certain kinds of social or other research. This need not be a one-way transfer process, since formation of a new department might well involve assignment of certain functions to the military, as mentioned before in the case of nuclear ordnance. More systematic coordination and congruence of policy and program can be achieved by two major departments in balance than by one department on the military side dealing with assorted scientific and technical agencies on the civil side. Even a casual perusal of the numerous memoranda of understanding, working arrangements, and coordinating mechanisms between the DOD and NASA, for example, suggests the complexity of these interagency relationships. Complexity cannot be eliminated but it can be reduced. The logic here is even more persuasive as agencies wrestle with joint projects and interacting programs.

All the decisions as to the composition of the Department of Science and Technology need not, of course, be made at one time. If the universe of government agencies is surveyed and all possible candidates identified, then problems of transfer would seem too overwhelming for immediate solution. The important first step is to assemble the independent agencies and subagencies as the departmental core, and then to build around them. This in itself will be a monumental task, but the vision of the National Academy committee of 1884 may still be sound (*48*).

References and Notes

1. Act of 7 July 1884, 23 Stat. 219. The commission, composed of three members each from the House and Senate, was known as the Allison Commission after its chairman, Senator William B. Allison of Iowa. Lyman served as one of the House members on the commission until the end of the 48th Congress on 3 March 1885. He was defeated for reelection.
2. *Congr. Rec.* **15**, 6175 (7 July 1884).
3. The NAS committee's report was transmitted to Lyman by O. C. Marsh, president of the National Academy of Sciences, by letter dated 16 Oct. 1884. It was printed in Senate Misc. Doc. No. 82 (serial No. 2345, 49th Congress, 1st session (1886) vol. 4; also as appendix D to the *Report of the National Academy of Sciences for 1884* (Government Printing Office, Washington, D.C., 20 April 1885), p. 33.
4. The NAS committee proposed that the commission include the president of the National Academy of Sciences; the secretary of the Smithsonian Institution; two nongovernment civilian scientists of high reputation appointed by the President of the United States for 6-year terms; one officer of the Corps of Engineers; one Navy professor of mathematics skilled in astronomy (the last two to be designated by the President for 6-year terms); the superintendent of the Coast and Geodetic Survey; the director of the Geological Survey; and the officer in charge of the Meteorological Service. The secretary of the department including the science agencies would be ex officio president of the commission, and the commission would be attached to the office of the secretary.
5. Senate Rep. No. 1285, 49th Congress, 1st session (8 June 1886), p. 54.
6. See A. Hunter Dupree, *Science and the Federal Government* (Harvard Univ. Press, Cambridge, 1957), p. 215. Dupree writes of this period: "In contrast to the glorious and successful defense of the new scientific bureaus, the experts had done a ragged job for a Department of Science. The National Academy had done nothing to push the brainchild of its committee, which admitted political defeat in advance" (p. 230).
7. *Congr. Rec.* **92**, A14 (1 Feb. 1946). The Luce bill provided for a Secretary appointed by the President with Senate confirmation, and five Assistant Secretaries, appointed by the President, to head, respectively, the following bureaus: Physics and Mathematical Sciences, Public Health and Social Sciences, Scientific Education and Information, Biological Sciences, and Engineering and Technological Sciences. The Secretary would be empowered to appoint an advisory council of not more than 100 members representing all branches of science.
8. *Science—the Endless Frontier*, A Report to the President on a Program for Postwar Scientific Research (July 1945). The report was reprinted by the National Science Foundation (Government Printing Office, Washington, D.C., July 1960).
9. *Congr. Rec.* **93**, 10567 (17 Nov. 1947). See Don K. Price, *Government and Science* (New York Univ. Press, New York, 1954), p. 48.
10. *Science and Public Policy*, Report of the President's Scientific Research Board (Government Printing Office, Washington, D.C., 4 Oct. 1947), vol. 3, p. 23.
11. Public Law 81-507, 64 Stat. 149 (10 May 1950).
12. *Progress Report on Science Programs of the Federal Government*, Senate Rep. No. 2498, 85th Congress, 2d session (9 Sept. 1958), p. 14; *Congr. Rec.* **105**, 1078 (23 Jan. 1959).
13. Public Law 85-567, 72 Stat. 426 (29 July 1958).
14. Killian's address, made on 29 Dec. 1958, was printed in *Science Program—86th Congress*, Senate Rep. No. 120, 86th Congress, 1st session (23 March 1959), p. 3.
15. D. K. Price, *Government and Science* (New York Univ. Press, New York, 1954), p. 63. Price was discussing the potential role of NSF as a central science agency and not specifically a Department of Science.
16. Senate Rep. No. 120, 86th Congress, 1st session (23 March 1959), p. 26. Berkner's views were set forth in an address, "National Sci-

ence Policy and the Future," at Johns Hopkins University (16 Dec. 1958), published in the same report, appendix D, p. 110. Brode's address on the same subject as retiring president of the AAAS (28 Dec. 1959) was placed in the *Congressional Record*, along with press articles and editorials, by Senator Kefauver [*Congr. Rec.* **106**, 615 (18 Jan. 1960)]. For additional materials on the pros and cons of a department, see *Science and Technology Act of 1958*, Analysis and Summary by the Staff of the Senate Committee on Government Operations, Senate Doc. No. 90, 85th Congress, 2d session (April 1958).

17. Senate Rep. No. 120, 86th Congress, 1st session (23 March 1959), p. 29.
18. *Create a Department of Science and Technology*, hearings before the Subcommittee on Reorganization and International Organizations, Senate Committee on Government Operations, 86th Congress, 1st session, on S. 676 and S. 586 (16-17 April 1959), pt. 1, pp. 47 and 71.
19. *Establishment of a Commission on a Department of Science and Technology*, Senate Rep. No. 408 (18 June 1959), p. 6.
20. *Ann. Amer. Acad. Polit. Soc. Sci.* (Jan. 1960), p. 27; Senator Humphrey placed this article in the *Congressional Record* [**106**, 5235 (10 March 1960)].
21. *Organizing for National Security: Science Organization and the President's Office*, staff study by the Subcommittee on National Policy Machinery, Senate Committee on Government Operations, 87th Congress, 1st session (committee print, 14 June 1961), p. 1.
22. House Doc. 372, 87th Congress, 2d session (29 March 1962).
23. Senator Humphrey placed an excerpt from the Stover report in the *Congressional Record* [**108**, 11822 (27 June 1962)]. A statement by Stover supporting the commission proposal was printed in *Establishment of a Commission on Science and Technology*, Senate Rep. No. 1828, 87th Congress, 2d session (6 Aug. 1962), p. 50. The report *The Government of Science* proposed that the Department of Science and Technology absorb the activities of NSF, PSAC and FCST, along with the Weather Bureau, National Bureau of Standards, Office of Saline Water, Coast and Geodetic Survey, Navy Hydrographic Office, Naval Observatory, portions of the Smithsonian's research work, and the Antarctic programs of the Navy and NSF. Larger agencies such as NASA and NIH were cited as candidates for inclusion, though AEC was excluded on the ground that its size and operational character could overwhelm the new department.
24. *Create a Comission on Science and Technology*, hearings before the Senate Committee on Government Operations, 87th Congress, 2d session, on S.2771 (24 July 1962), pt. 2; *Reorganization Plan No. 2 of 1962*, hearings before a subcommittee of the House Committee on Government Operations, 87th Congress, 2d session (17 April 1962). Jerome B. Wiesner wrote later: "Possibly the most important consequence of providing a statutory basis for the scientific activities in the Executive Office of the President is that the Director may now appear before Congress to explain, when possible, the Government-wide views of activities and problems" [*Where Science and Politics Meet* (McGraw-Hill, New York, 1965), p. 47].
25. *Systems Development and Management*, hearings before the Military Operations Subcommittee of the House Committee on Government Operations, 87th Congress, 2d session (1 July 1962), pt. 1, p. 156.
26. *Establishment of a Commission on Science and Technology*, Senate Rep. No. 1828, 87th Congress, 2d session (6 Aug. 1962).
27. *Congr. Rec.* **108**, 15968 (8 Aug. 1962).
28. Senator McClellan placed excerpts from the Wiesner testimony in the *Congressional Record* on two separate occasions [**109**, 2395 (18 Feb. 1963) and *ibid.* (23 May 1963), p. 9299]. It was also carried in the committee report cited below (*29*).
29. *Establishment of a Commission on Science and Technology*, Senate Rep. No. 16, 81st Congress, 1st session (4 March 1963).
30. *Congr. Rec.* **109**, 3808 (8 March 1963). In remarks accompanying the bill Senator Humphrey said an independent "Hoover-type" commission was needed to (i) counterbalance the executive's excessive dependence on a small in-group of scientists for policy advice and program evaluation; (ii) review, with the aim to improve, the activities of the NAS-NRC as well as those of the government agencies; and (iii) examine federal organization for information retrieval.
31. Referring the bills to the House Committee on Science and Astronautics signified a change in jurisdictional policy. Heretofore such bills had been referred to the House Committee on Government Operations, which generally has jurisdiction over organization matters.
32. *National Goals and Policies*, House Rep. No. 1941, 80th Congress, 2d session (29 Dec. 1964), p. 49. The Select Committee expired with the 88th Congress on 3 Jan. 1965. In accordance with one of its recommendations, a Subcommittee on Research and Technical Programs was established within the House Committee on Government Operations. This subcommittee, chaired by Representative Reuss, was in existence through the end of the 90th Congress.
33. R. Lapp, ***The New Priesthood*** (Harper & Row, New York, 1965), p. 204. Lapp proposed that the Department of Science make basic research grants (on a lump-sum basis); manage the government laboratories; absorb all or part of the functions of OST, PSAC, and FCST; and take over the functions of the AEC (civilian part), NSF, ONR, Office of Saline Water, National Bureau of Standards, and the Weather Bureau.
34. *Establish a Commission on the Organization and Management of the Executive Branch*, hearings before the Subcommittee on Executive Reorganization of the Senate Committee on Government Operations, 90th Congress, 2d session (23 Jan. 1968), p. 58. Hollomon proposed that the Department of Science and Technology include the NSF, NASA, ESSA, National Bureau of Standards, Geological Survey, Census Bureau "and perhaps parts of NIH and the AEC."
35. D. F. Hornig, remarks at AAAS Meeting, Dallas, Texas (29 Dec. 1968).
36. Senate Misc. Doc. No. 82 (serial No. 2345), 49th Congress, 1st session (1886), vol. 4, p. 66.
37. The usual text for this organizational approach is the report of the First Hoover Commission, "General Management of the Executive Branch" (Feb. 1949), which recommended that: "The numerous agencies of the executive branch must be grouped into departments as nearly as possible by major purposes in order to give a coherent mission to each Department" (p. 34). Senator Humphrey quoted this recommendation in his *Annals* article (*20*).
38. The Department of Housing and Urban Development actually was established during the Johnson Administration, although President Kennedy pressed for its creation from the beginning of his administration. The stumbling block to congressional acceptance was President Kennedy's announced intention to appoint Robert C. Weaver as Secretary of the new department. Similarly, congressional opposition to a putative department head (Oscar R. Ewing) prevented President Truman from getting the Department of Health, Education, and Welfare, which was created in the Eisenhower Administration.
39. President Nixon announced the appointment of an Advisory Council on Executive Organization on 5 April 1969. The members are: Roy L. Ash (chairman), president of Litton Industries, Inc.; George Baker, dean of the Graduate School of Business Administration, Harvard University; John B. Connally, former governor of Texas; Frederick R. Kappel, chairman of the executive committee, American Telephone and Telegraph Company; and Richard M. Paget, member of Cresap, McCormick and Paget.
40. Lapp (*33*, p. 206) proposed that the AEC's nuclear production facilities be mothballed in part and the remainder transferred to the Department of Defense. Representative Craig Hosmer, in an address "The Science Establishment: Where Is It Headed?" [*Congr. Rec.* (6 March 1968), p. E1606] posed the AEC problem in terms of diversification or decline: "Unless AEC's charter is revised to give it a responsibility to conduct research for other government agencies, it would seem that some of these facilities and programs would be better off under an organization more fundamentally oriented toward basic research, such as the National Science Foundation."
41. R. E. Miles, **Jr., *Public Admin. Rev.* 27**, 1 (March 1967). Text included in hearings before the Subcommittee on Executive Reorganization, Senate Committee on Government Operations, 90th Congress, 2d session (23 Jan. 1968), p. 115.
42. Public Law 90-407, 82 Stat. 360 (18 July 1968).
43. Public Law 85-454, 80 Stat. 203 (17 June 1966). See also Public Law 90-242, 81 Stat. 780 (2 Jan. 1968).
44. *Our Nation and the Sea*, Report of the Commission on Marine Science, Engineering and Resources (9 Jan. 1969 preprint), p. 7.
45. ". . . The easy answer to all problems in Government, scientific and nonscientific, seems to be to move them closer to the President. I don't think that tenable for all things—he is already overburdened" [D. F. Hornig (*35*)].
46. *State of the Union Message*, House Doc. No. 1, 90th Congress, 1st session (19 Jan. 1967), p. 3.
47. A current example of a government laboratory with diversified scientific capabilities and no obvious place to go upon withdrawal of military sponsorship is the Navy Radiological Defense Laboratory. It is slated for closure by the end of this year, even though its resources could be readily adapted to important research in the civil sector. The proposed closure of NRDL also illustrates the poor planning not infrequently found in government. Six months ago a $6-million cyclotron was installed for special research in biomedical effects of radiation.
48. The views expressed herein are the author's and not necessarily those of any member of the Congress.

Congress and Science Policy: The Organizational Dilemma

RICHARD L. CHAPMAN

In less than a decade, Congress has conducted special investigations into federal support of research and development through three ad hoc committees, set up two new standing committees, and created two new subcommittees—all in an effort to probe broad policy questions of science and technology. Yet, the fundamental problem of devising an effective science policy continues to baffle Congress and the White House. Perhaps what is needed is a Science of Science Policy. Here, Richard L. Chapman, senior staff member, National Academy of Public Administration, examines the problem and indicates a solution.

The past decade bears witness to an intensified congressional interest in science and technology as well as in their impact upon national policy. The growth of this interest can be measured by the number of congressional committees or subcommittees that have conducted investigations into some aspect of science and technology. The seemingly disorganized manner of the legislative branch in dealing with national questions of science policy—as with nearly any other broad question—has bred an increasing frustration, not only among some legislators, but among interested members of the public—especially the research and development community.

World War II had barely ended when the Senate Committee on Military Affairs opened hearings on the creation of a National Science Foundation (NSF). A number of legislators and scientists, prominent in the war-developed Office of Scientific Research and Development (OSRD), were eager to see that the conclusion of the war did not end the highly successful cooperation between the federal government and science, both academic and industrial.

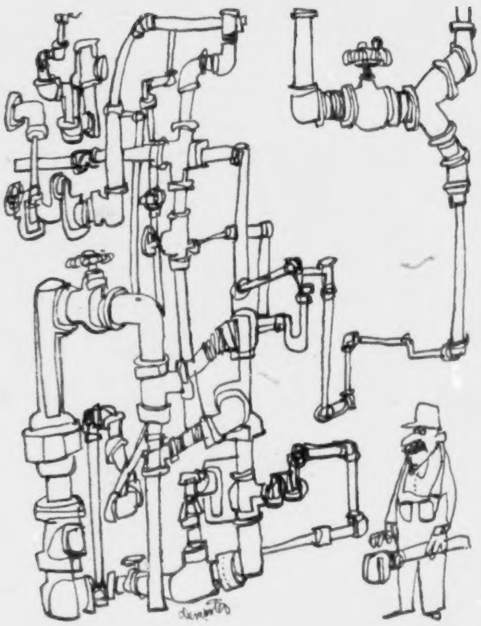
The first major step taken by Congress to involve itself on a continuing basis in scientific matters was the establishment of the Joint Committee on Atomic Energy by the Atomic Energy Act of 1946. The Committee quickly achieved control in determining policy matters on atomic energy, developing an interest and exercising authority even in details of administrative management.

The next research and development foray by Congress coincided with the national furor over the successful launching of Russia's sputnik in October 1957. Most congressional concern focused upon the position of the United States vis-a-vis Russia with respect to missile and space technology. The Senate, through its Armed Services Preparedness Investigating Subcommittee, pursued the cause and solution to the missile and space mess. Hubert Humphrey, then Chairman of the Senate Government Operations Subcommittee on Reorganization and International Organizations, introduced legislation to create a department of science to improve the coordination and effectiveness of federal science organizations. Both the Senate and House established high-powered select committees to develop new legislation to put the United States back into the space race.

The major objective of Senator Humphrey's proposed Science and Technology Act of 1958 was to organize, under a secretary at the Cabinet level, the coordination and centralization of those federal science activities that were scattered among various departments. Humphrey emphasized that he wished to give science a more powerful voice in government through a cabinet-level position, and to provide Congress with access to scientific expertise in the Executive Branch. The legislators had been somewhat miffed at their inability to get top-level scientific opinion from the newly-appointed President's science advisor and his committee, whose position of executive privilege put them beyond the reach of congressional committees, and seemingly denied the Congress access to outstanding scientists familiar with major federal programs.

EVERYONE IN THE ACT

While Senator Humphrey was promoting a Department of Science, the House and the Senate select committees tackled the creation of a new civilian space agency. At the same time, the Joint Committee on Atomic Enegry was ready with questions about why the Atomic Energy Commission (AEC) might be a good framework within which to pursue a national



space mission. With the creation of the civilian National Aeronautics and Space Administration (NASA), both the House and Senate established new standing committees to exercise legislative jurisdiction. In the Senate it was the Committee on Aeronautical and Space Science. The House created the Committee on Science and Astronautics, implying an interest in science in general, and specifically including both NASA and NSF.

The traditional House and Senate committees found reasons to conduct investigations into some aspect of science and technology. For example, the Armed Services Committees of both the House and Senate conducted special investigations into the military missile program, as did the House Military Operations Subcommittee, and the Senate Government Operations Subcommittee on National Security.

The Bureau of the Budget had provided a new vehicle for focusing attention on federal research and development when it issued a special analysis of federal research and development programs as an appendix to the President's budget for fiscal 1955. Although this special analysis continued to be no more than a collection of statistics on the research and development financed by federal agencies, it helped to emphasize the aggregate importance of federal research activity.

For nearly six years Congress had been concerned about the need to accelerate research and development spending. But by 1963 the expansion was moving rapidly, and spending increasing so quickly that Congress began to consider the question of whether or not federal money was being wisely spent. The House established the Select Committee on Research on September 11, 1963. Over the next 16 months this committee, chaired by Carl Elliott (D., Ala.), issued 10 studies ranging from a survey of federal research and development grant administration practices, through contract policies and procedures, to an assessment of the impact of federal research and development programs. The work of the committee was widely hailed for contributing to better understanding of the breadth, intensity, and nature of research and development programs conducted or sponsored by federal agencies. The Eliott Committee left a substantial legacy of continuing congressional interest in better means to cope with the multitude of federal research programs, their coordination, relative priorities, appetite for national resources, and general impact.

These same concerns moved the House Science and Astronautics Committee to establish the Subcommittee on Science, Research, and Development under the chairmanship of Emilio Q. Daddario (D., Conn.). Although the jurisdiction of the Committee was limited to NASA and NSF, it undertook to lay at least joint claim to broad, general questions of science and government. It did so by launching a series of hearings in October and November 1963 on the topic of government and science.

HOW IT GREW

The Elliott Committee had suggested the need for more accessible and independent scientific advice for Congress. Late in 1964 Congress established the Science Policy Research Division within the Legislative Reference Service of the Library of Congress. This new Division was to provide the reference service, staff support, and research reports for congressional committees on scientific and technical questions. It was also to provide means for Congressmen to obtain technical advice from a staff source available to the whole Congress, yet independent of the executive branch.

Another recommendation of the Elliott Committee was for creation of a new subcommittee to deal with science and technology in the House Committee on Government Operations. This recommendation was fulfilled in the first session of the Eighty-Ninth Congress when the Committee on Government Operations established the Subcommittee on Research and Technical Programs, naming Henry S. Reuss (D., Wis.) chairman. The new subcommittee's first investigation was on the policy question of conflicts between the federal research programs and the nation's goals for higher education.

Following the example of its sister committee in the House, the Senate Committee on Government Operations established the Subcommittee on Government

Research to undertake studies and hearings "into the operation of research and development programs financed by departments and agencies of the federal government." The new subcommittee conducted its maiden investigations into federal support of international social science research and the geographic distribution of federal research funds to academic institutions.

In less than a decade Congress had pressed its interest in science and technology by (1) conducting special investigations through three ad hoc committees, (2) establishing two new standing committees in the field of space sciences and technology, and (3) creating two new subcommittees to probe broad policy questions of federal science and technology and to oversee the efficiency and economy of the multitudinous federal research and development programs.

R&D DIVERSITY

Almost every federal agency of any importance has some type of research or development program, whether it is conducted in the agency's own laboratories or through grant or contract with nongovernmental institutions.

A 1966 Legislative Reference Service study for the House Government Operations Committee indicated that federal agency laboratories accounted for an average of 22 per cent of all obligations for research and development over the decade 1956–66. These laboratories employed some 300,000 scientists, engineers, and technicians. Their subject matter included the entire scientific and technical universe, ranging from the most fundamental research to testing and evaluation following engineering development.

Another barometer of this diversity is that the Eighty-Eighth and Eighty-Ninth Congresses published nearly 1,900 documents dealing with research and development on subjects from agriculture to urban affairs.

This vast array of federal scientific activities, combined with the broad and equally varied interests of legislators, suggests that it might be profitable to find some unity or to enforce some degree of common direction. With the exception of NSF, research and development is usually undertaken as a means toward accomplishing an agency's goals—whether or not the project can be classified as basic research, applied research, or is developmental in nature. The important question is: When programs are grouped across departmental lines in order to view science and technology as a whole, is the perspective of *purpose* distorted by removing these various projects from the organizational context in which they had identity with agency or departmental goals? Many projects appear silly or worthless when viewed away from the context of departmental programs to which they are supposed to relate. There is obvious value in aggregating similar research projects to understand their total size, scope, and impact, but Congress has a tendency to pick at minor details which may seem to provide examples of inefficiency or duplication—and which may be isolated instances—while neglecting broader policy questions.

One can see the dilemma: legislators would like to view the broad sweep and impact of federal research and development programs in order to provide legislative or organizational means for improved coordination and the most effective use of scarce resources. But concurrently, they face the problem of evaluating research within the context of the agency mission toward which it contributes.

To make the problem more difficult, analysis of the impact of science and technology within our economy has been so limited that we have no recognized standards or measures except for gross statistics on total dollars, scientific-technical personnel, facilities, and projects by various subject-groupings. Here, again, there is the question of conflict among differing values. Can satisfactory value judgments be made about the contribution of projects to the program goals of specific agencies, while their worth is also tested with respect to their contribution to national policy goals in science and technology?

MICRO CONSIDERATIONS

I believe that the answer lies, not in trying to combine these disparate values in the same congressional review process, but in taking those steps necessary to assure adequate recognition of both the "micro" and "macro" considerations. The budget-appropriations procedure—both executive and legislative—provides a means for raising the relevant "micro" questions. No regular procedure has yet been developed for the periodic review of broad national questions on science and technology, either by the Executive or by Congress.

The first session of the Ninetieth Congress is an excellent example of the pluralism of congressional interest and authority on matters of science and technology. For example, the Senate had 12 committees and 27 subcommittees that were frequently involved in either the appropriation, legislation, or oversight of research and development activities. The Senate also had four committees or subcommittees that dealt exclusively with research and development. In the House there were 13 committees and 37 subcommittees that frequently or occasionally dipped into scientific questions, and three committees or subcommittees with specific responsibility in science and technology. In addition there was the Joint Committee on Atomic Energy with its three subcommittees handling questions of nuclear research or related technology.

In the past, Congress seems to have done reasonably well in dealing with major national questions on research and development. For instance, control of research and development and the production of atomic materials or devices was centralized in the AEC for both military and civilian purposes. Continuing congressional oversight was assured through the Joint Committee on Atomic Energy. Congress has strongly encouraged missile and space research and development through committees concerned with the military or civilian programs, and has promoted health research through the National Institutes of Health in a more or less orderly fashion, based upon the strong interest of two key appropriations subcommittee chairmen. Stronger emphasis on air and water pollution research has been systematically pushed by aggressive committee chairmen.

CROSSING LINES

Most of these efforts reached their peak of effectiveness when single committees or subcommittees in the House and Senate actively promoted a major program or a particular agency's interests. As resources become more scarce and legislators become more critical of federal research, it is questionable whether or not Congress can coordinate its committee action when programs cross committee lines or affect competing interests—either congressional or executive.

Some simple case examples illustrate the problem that Congress will increasingly face in the future.

In the field of oceanography, 14 different executive agencies had a research stake in 1961. The subject was of special interest to the Senate Committee on Commerce and to the House Committee on Merchant Marine and Fisheries. The authorizing and appropriations subcommittees responsible for each of the 14 agencies also had an interest, but from the perspective of how the oceanographic projects rated in the priority scale with respect to the other agency responsibilities over which these committees and subcommittees maintained legislative or appropriations control. The result was that, although the agencies were able to coordinate their programs, even agreeing on their respective contributions to financing, the integrated program was not treated as an entity when it went to Congress. Some portions were generously treated and others were substantially reduced. The aggressive attempt on the part of the House Committee on Merchant Marine and Fisheries to establish a controlling interest in oceanography as a single national program failed because of the jurisdictional jealousy of affected committees and subcommittees.

A similar case was that of the 1961–63 weather satellite program jointly conducted by NASA and the Weather Bureau. Most of the satellite funding was by NASA, while the Weather Bureau was to finance the operating system. In addition to flying weather satellites to test the whole system, NASA used them to test Weather Bureau-developed and financed sensor instruments. The Weather Bureau used test data from the research satellites to improve operational weather forecasts. The financing of the operational and research aspects was so intertwined that adverse congressional reaction to either agency's financing could disrupt it.

NASA went through the authorization process as part of the annual procedure of obtaining funds from Congress. In the House this meant appearing before the House Science and Astronautics Committee which was generally favorable to the weather satellite program. NASA then appeared before the House Appropriations Subcommittee on Independent Offices where it also received a reasonably enthusiastic hearing, though the weather satellite program was only about $60 million in a multi-billion dollar request, so that it was rarely visible. On the other hand, the Weather Bureau, having permanent authorization, sought its funds directly from the House Appropriations Subcommittee on the Departments of State, Justice and Commerce, the Judiciary, and related agencies. Here it ran up against economy-minded John Rooney (D., N.Y.), who was a good deal less friendly to the Weather Bureau portion of the program than was his colleague, chairman of the Independent Offices Subcommittee, Albert Thomas (D., Tex.), who reviewed the NASA portion of the program.

There was no coordination or communication, either among the chairman, members, or staff of the appropriations subcommittee or the authorization subcommittee concerned. Thus a "national" program involving a combined scientific effort on the part of two different agencies could be coordinated only until it reached the Hill. Congress did not view it as an integrated whole.

THE EXECUTIVE ROLE

Congress has been unable to develop or provide guidance on science policy except as its committees are able to influence the Executive Office of the President (especially the Bureau of the Budget and the Office of Science and Technology), picking away on ad hoc issues.

The executive branch plays an important role in how Congress is organized, or how Congress attends to the principal matters of public business, by the manner and types of problems or programs that the executive presents for legislative consideration. At least part of the answer can be found in the observation of William D. Carey, Assistant Director of the Bureau of the Budget, that a substantial part of the problem rests at the executive end of Pennsylvania Avenue. The Executive Office of the President, in spite of its high-powered Office of Science and Technology, Council of Economic Advisors, and Bureau of the Budget, has yet to cope successfully with broad science and public policy issues in a systematic fashion that would provide some basis for Congress to evaluate policy proposals and their alternatives. Indeed, some credit must be given to critics in the Congress for prodding the Executive Office into taking some action—if only to collect information and study major policy issues, such as establishing the

Committee on Academic Science and Engineering to probe the impact of federal programs with respect to scientific manpower and university programs.

Any attempt to provide a more integrated congressional review of U.S. science policy immediately confronts two obstacles: (1) the fact that the Executive Branch has yet to develop anything resembling a "science policy" itself; and (2) the reluctance of Congress to vest central authority for science and technology in a single committee when over 70 different committees and subcommittees have at least some interest in research and development.

Of course, there is a science policy, even though it has not been explicitly stated through legislation or in official documents. What we have is a collection of legislation, administrative rules, and general practices that is the result of virtually undirected growth.

The Office of Science and Technology, the Federal Council for Science and Technology, and the National Science Foundation have, from time to time, studied those broad issues that have grown out of the vast collection of federal research and development programs. For example, all three have been concerned with various aspects of manpower. One or more of these groups have been concerned about the inadequacy of federal pay or other emoluments for attracting first-rate scientists and engineers into federal laboratories. On other occasions they have turned their attention to the general availability of manpower resources. Rarely have these organizations given consistent, comprehensive attention to the impact of programs as they multiply and expand.

The manpower problem only illustrates how the Executive Office has failed to provide a coordinated general framework for research and development activity. What has been produced is a collection of ad hoc analyses, studies, and recommendations on programs that had or were expected to have general policy implications. Some examples have been weapons systems development, a nuclear test-ban treaty, and the problem of environmental pollution. But such studies do not provide the necessary perspective from which to judge the broad allocation of resources for science and technology.

Several proposals have been advanced by which Congress might more adequately deal with broad questions of science and technology. The Daddario subcommittee recommended the use of ad hoc study groups composed of members of those congressional committees concerned with particular programs, who could meet to survey the total impact of the program. Presumably the members would report back to their respective committees, helping to provide them with a comprehensive view of the interagency program prior to final action on specific legislation.

A second Daddario proposal was for the formal establishment of a technology assessment board that would be vested with the responsibility for providing "early warning" on potential dangers as well as the benefits flowing from new technology.

Senator Edmund S. Muskie (D., Maine) proposed a select Senate Committee on Technology and the Human Environment to conduct a three-year study entailing the collection and evaluation of information about technical change and how this change will affect the human environment. The Senate Committee on Government Operations favorably reported this resolution to the Senate in the fall of 1967.

More ambitious was the bill introduced by Senator Gordon Allott (R., Colo.) calling for the establishment of a Joint Committee on Science and Technology and requiring an annual Presidential report on federal programs in science and technology that would be referred to this joint committee. The purpose of the committee would be viewed as primarily educational—both for the public and for Congress. The committee would have no legislative jurisdiction, and thus would pose no particular threat to the jurisdiction of existing committees or subcommittees, yet it would have a responsibiilty for responding to the annual report on science and technology by the President. Even though the committee would not have the authority to control programs it should be able to exercise influence (similar to that of the watchdog Government Operations Committees in both the House and Senate) by publicizing major problems, deficiences, and inequities that result from poor coordination or from outright rivalry—either in the Executive or the Congress. It could also direct attention toward future expectations and the impact of science and technology on other governmental activities.

The Allott bill received some support in the Senate, but virtually none in the House. Perhaps some of the congressional reluctance toward central or cooperative committee jurisdiction could be overcome if Congress were faced with a strong initiative on the part of the executive. If the President presented the Congress with a report on the status of science and technology similar to his annual economic message, Congress would have to react to this report though it would retain the option of the method by which it chose to react.

In any case, we are most unlikely to see anything approaching a coordinated or consolidated approach toward science and technology in the Congress until the executive branch takes the initiative to provide some general science policy framework. William D. Carey, Assistant Director of the Bureau of the Budget, has long advocated such a Presidential report on research and development. The Elliott Committee and the House Committee on Government Operations both have recommended such a report. The real question is: When can we expect the Executive Office of the President to respond to this need?

National Research Council: And How It Got That Way

In the last week of April the members of the National Academy of Sciences (NAS) will make their annual migration to Washington. They will spend much of their time in the agreeable ceremonial labors of electing new members to perpetuate their society, bestowing honors, and attending scientific sessions. But this year, on and off the agenda, the members must confront the question of how better to carry out their congressionally chartered responsibilities of providing advisory services to the government.

The NAS meets from 26 April through 29 April, and the National Academy of Engineering (NAE) will follow with its own annual meeting in the Academy's marbled halls on 29 and 30 April. The order and timing of the meetings might be taken as symbolic of the separate but equal status accorded NAE when it was organized in 1964 under the NAS charter. The NAE program will be similar to that of the NAS, but the engineers' mood is likely to be that of an exasperated younger brother who feels his talents and energy are misused in the family business.

A major topic of concern at both meetings will inevitably be the National Research Council (NRC), the operating arm of the NAS and NAE, through which the Academy performs its advisory functions. The NRC has a staff of about 1000 and an operating budget of roughly $30 million this year. NRC performs no laboratory research, of course, but is essentially a vast, *sui generis* committee system drawing on the voluntary services of as many as 9000 American scientists, engineers, and other professionals—which makes it, all in all, the biggest consulting firm in the world.

NAS-NAE-NRC, to use its full, not very brief abbreviation, is replete with paradox. The parent NAS is a unique hybrid, a congressionally chartered, private, nonprofit organization incorporated in the District of Columbia. The government provides no direct subsidy and exercises no oversight authority, but 80 percent of the Academy budget comes from government sources. Perhaps the richest paradox involves the honorary aspects of the Academy. For the individual, membership in the Academy certainly signifies making it in American science. But it is really the NRC which discharges the advisory obligations imposed by the charter. There is nothing in it about the Academy being an honorary society.

Nevertheless, although a minority of Academy members are extensively engaged in NRC activities, it is the prestige of Academy members that gives the organization its unique standing. And, significantly, "Academy" is the generic term commonly used for NAS-NAE-NRC and all its works. If the Academy is not above suspicion or beyond reproach, it remains the court of last resort on scientific and technical questions.[*]

Since World War II, however, as the activities, budget, and staff of the NRC expanded, some Academy members began to question the appropriateness and the quality of work done by NRC committees in the name of the Academy. Uneasiness centered on the growth and what appeared to be the increasing independence of the NRC bureaucracy.

In the 1960's, as a result of American involvement in the Vietnam war and the emergence of environmental and consumer-protection issues, it became more difficult to separate the technical content of some problems from their social and political aspects. Critics inside and outside the Academy argued that, by responding to narrowly defined requests for advice without commenting on the broader implications of the issues involved, the Academy was sometimes being used to lend respectability to socially dubious activities.

The most biting public criticism of the Academy to date came in remarks by former Interior Secretary Stewart L. Udall at the AAAS meeting in December. Udall's comments at a panel session were directed at scientists in general. He said, "At worst, many men of science are allowing their findings to be used as buttresses for status quo thinking, and unnecessarily accepting a backseat 'technician's' role in which their larger opinions about the American future are not even sought."

Udall then focused his attack on the Academy directly, charging that "By confining itself to a clientele almost exclusively made up of government agencies—and by permitting its clients to phrase the questions it will study—the Academy has all too often become a mere adjunct of established institutions."

NAS president Philip Handler counters with an *ad hominem* retort to Udall's criticism, saying that if, while he was still at Interior, "Mr. Udall had implemented the reports to him from here, he would have been a great hero." But Handler, who has sustained a reformist image since he took over the Academy's top office in July 1969, does not dismiss the criticism of Udall or others out of hand. "As a generality it won't do," he insists. "You'd have to have an extensive appraisal of multiple engagements of the Academy. The extent of the involvement is greater than the critics and even the membership know. But that still leaves truth in the accusation that we haven't led the pack."

Other officials of the organization feel that the critics ignore what has been happening at the Academy in the way of changes in attitudes and specific reforms, including the beginning of a restructuring of NRC.

Perhaps the chief difficulty in evaluating criticism of the Academy lies in the nature and diversity of its work. NRC's product is advice, and it has no responsibility for implementing that advice. Furthermore, with its eminence, the question that arises with the Academy is, Who is to judge the judge?

The problem is compounded by the sheer volume of NRC activity—between 400 and 500 committees—the great diversity in styles and the degree of decentralization and authority that exists in an operation depending so heavily on volunteers. Like many loosely structured organizations, NRC is governed more by habits and attitudes than by rules, and it is important to understand how these habits and attitudes developed.

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[*] Three articles by D. S. Greenberg in *Science* (14, 21, and 28 April 1967) provide extensive background on Academy problems and politics, and, more recently, two articles in the *National Journal* (16 and 30 January 1970) by Claude E. Barfield marshal considerable detail on the operations of NAS-NAE-NRC.

NRC was created in 1916 when it became clear that the Academy was too narrowly based to respond adequately to the wartime emergency. NRC's performance was sufficiently creditable to win it permanent status, and between the wars it was moderately active on the advisory front and at the same time attempted with some success to bolster the welfare of science, principally through soliciting support from private sources.

With the coming of World War II, Academy luminaries such as Bush, Conant, Compton, Tolman, and Jewett took prominent parts in the great wartime mobilization of scientists and engineers, but the NRC was relegated to a secondary role by the rise of the National Defense Research Committee and then the Office of Scientific Research and Development.

After the war the scientific leadership, their influence considerably strengthened, clearly saw a place for the NRC on the "endless frontier" which seemed to be opening for science. The leadership had sour memories of the lean years for science before the war. Now, the government needed scientific counsel and had money. The scientists had expertise and needed support. And so a mutually advantageous *quid pro quo* was worked out. And, because the scientists' relationship with the government during the war had been regarded as both productive and patriotic, not many questions were asked.

The demand for advisory services multiplied in the postwar years as new agencies like the Atomic Energy Commission and later the National Aeronautics and Space Administration were established to do the government's work in new fields. And the requests for help from old-line agencies with new technical and scientific problems also increased. At the same time the NRC grew increasingly active as advocate, agent, and broker for the scientific enterprise at home and abroad.

A key role in the period of growth went to Detlev W. Bronk, who was active in the wartime Air Force medical research board and went on in the 1950's and 1960's to the presidency first of Johns Hopkins and then of the Rockefeller University. Bronk became chairman of the NRC after the war and then in 1950 was elected to the Academy presidency. Partly in an effort to exert stronger Academy control over the NRC, the NRC chairmanship and NAS presidency were combined for the first time. Under Bronk, the NAS-NRC went through an expansionary period: it played a central role in international scientific affairs, epitomized by the organizing of the International Geophysical Year; it established the Academy's presence in new fields, as with the creation of the Space Sciences Board; and it encouraged new scientific enterprises, as when the Academy played the role of godfather to the American Institute of Biological Sciences.

During the years of growth the basic structure of the NRC altered very little. NRC was originally organized on the basis of divisions related directly to the disciplinary sections to which NAS members are elected. There are divisions of behavioral sciences, biology and agriculture, chemistry and chemical technology, earth sciences, engineering, mathematical sciences, medical sciences, and physical sciences. There are also two specialized "offices"—the Office of the Foreign Secretary (headed by the Academy's elected Foreign Secretary, Harrison Brown of Caltech), which handles the Academy's relations with foreign academies and international cooperative programs, and also an Office of Scientific Personnel, which deals with manpower problems and administers fellowship and grant programs.

As a matter of principle, NRC avoids entanglements with operating programs, but exceptions seem to keep exerting heavy pressure on rules in the NRC, and two big budget items at least bend the principle. The Highway Research Board (HRB), created after World War I, has been a favorite target of Academy critics. It is argued that most of the work done through the board, which controls a budget of some $5 million a year, is routine and "applied" rather than of a type which only the NRC is competent to perform. It is also suggested that the board has developed strong ties with the highway lobby and that the existence of the board, operating under the prestigious wing of the Academy, has actually retarded serious work on alternative forms of transportation. Defenders of the board say it has facilitated cooperation between state highway authorities, which would otherwise have been impossible, and not only has raised the level of highway technology but has done much to educate state and industry officials to the deleterious side effects of unbridled highway building.

A reappraisal of the NRC role in this sector is, in fact, under way, since plans are afoot for the creation of a new Division of Transportation which would incorporate the $3 million National Cooperative Highway Research Program and other HRB functions into a division designed to take a balanced approach to transportation problems. The new division would be the first to be organized on "functional" rather than disciplinary lines, and NRC officials say that other functional divisions will follow.

Another big budget item imposed on the Academy by historical circumstance is the Atomic Bomb Casualty Commission (ABCC). The commission was formed after World War II to make a longitudinal study of victims of atomic bombing in Japan in cooperation with the Japanese. In the atmosphere that prevailed, the prestige and nongovernment status of the Academy made it a desirable administrator of the commission. The question has been reviewed frequently, but the involvement has proved difficult to end (*Science*, 8 May 1970).

To outsiders, the typical NRC product is a report of a committee on a technical or science policy issue, which appears in a tasteful format bearing the Academy imprimatur. Academy activities, however, can't be stereotyped. Sometimes a single meeting will be called to discuss a problem. Sometimes a committee will go on for years and years discussing an abstruse, technical subject in meetings a couple of weekends a year conducted in the leisurely fashion that is one facet of the Academy style. Sometimes no report appears at all and sometimes, as in the case of the Drug Efficacy Study carried through under the aegis of the Division of Medical Sciences, hundreds of scientists will be enlisted in a large-scale, tightly organized review effort.

NRC's main relationships with the Department of Defense were, of course, established during World War II and its aftermath and are now the subject of increasingly critical attention. The proportion of the NRC total budget derived from military sources has declined over the years, so that contracts from the military services amounted to about $2.7 million of the total $25 million income from federal sources in the 1969–70 fiscal year. NRC officials estimate that classified work represents about $582,000, or roughly 2 percent of the NRC budget.

Since NRC committee chairmen and

Table 1. NAS-NAE-NRC income from contracts. These figures from the 1969–70 treasurer's report show income used under contracts and grants from federal agencies.*

Federal agency	Income
Department of Agriculture	$ 150,681
Department of Commerce	626,648
Department of Defense	
Department of the Air Force	484,444
Department of the Army	1,608,123
Department of the Navy	1,619,452
Department of Health, Education, and Welfare	1,941,990
Department of Housing and Urban Development	363,637
Department of the Interior	229,022
Department of State	1,117,702
Department of Transportation	3,294,411
Department of the Treasury	107,366
Executive Office of the President	315,001
Agency for International Development	758,853
Arms Control and Disarmament Agency	75,440
Atomic Energy Commission	4,150,511
Federal Communications Commission	27,456
Federal Radiation Council	17,179
General Services Administration	64,914
National Aeronautics and Space Administration	4,810,192
National Science Foundation	3,314,648
Smithsonian Institution	56,502
Veterans Administration	300,367
Total	$25,434,539

* Funds from private nonfederal sources amounted to $2,670,104, including $954,001 from state governments.

members are part-timers and volunteers, the influence of the staff on the quality of the NRC output is obviously crucial. Again generalizations are difficult. Differences between divisions seem to be wide, and the abilities and responsibilities of staff members appear to cover a full range. Some staff men seem to do little more than handle bookkeeping and correspondence, while, at the other extreme, some secretaries of committees participate as equals in a committee's deliberations, shape the agenda, and even write the reports.

By the most recent count, the staff numbers 913 people. Of these, 357 are classified as professional and managerial, and roughly 90 of them hold degrees at the doctoral level. The professionals fall into two major categories. The smaller group is made up of people brought in on a temporary basis from academic life, government agencies, or industry to staff a particuluar project. Normally they stay a year or two and then return to their point of origin or, often, to a better job. A fair number like the atmosphere, are assigned to work on a new contract, and stay on. The aim is to have "transient" staff members constitute about a third of the total, but the proportion now is well under that.

In the larger group of professionals in the "career" category, a fairly high proportion come from military or civil agency backgrounds. NAS salaries these days are roughly competitive with federal agency salaries and the NRC budget has continued to rise modestly during the current "recession" in science, but, since the NRC's mode of financing can imply job insecurity, the Academy staff has traditionally been congenial to military and government men who retired early and had pensions to supplement their incomes. Observers say the staff has tended to be older and habituated to an orderly, bureaucratic life, but they also say that there are now signs that the Academy's involvement in socially and politically sensitive issues is attracting younger, more activist staff members.

NRC lives on grants and contracts, and observers sometimes suggest that the staff spends a good deal of time out beating the bushes for work and that some develop a mutually beneficial and protective relationship with their opposite numbers in federal agencies.

Nobody this reporter talked to at the NRC pretended that most proposals from agencies are spontaneously generated and arrive as a surprise to the Academy. A formal letter request does ultimately appear addressed to the president, but usually discussions have gone on with NRC staff members involved to some degree. Often a staff member originates the idea and sometimes even drafts the agency letter.

To the question of whether some NRC staff members have developed ties with federal agencies, NAS president Philip Handler acknowledges that a "buddy system" does exist, with some staff members working with "counterparts" in client agencies. Handler notes that this is "true in universities equally." The important thing, says Handler, is that the proposals are subjected to adequate scrutiny before work is accepted. The primary guardian is the executive committee of the division made up of professionally competent outsiders. If a proposal is approved, the project must get the blessing of the Academy council, which is made up of Academy members and is the governing board and conscience of the Academy.

Handler says when he assumed the presidency of the Academy he found a number of members who were concerned about the growth of the NRC and "didn't know what it did." The exercise of control by the Academy of the NRC has been an issue almost from the creation of NRC. Concern about this was an element in the decision to combine the office of NAS president and NRC chairman when Bronk took over in 1950. It was a strong factor in the move to make Frederick Seitz, Bronk's successor, the first full-time president. And Handler was elected to the presidency with an implicit mandate to modify the structure and management of the NRC.

The problems facing Handler in carrying out his mandate are formidable. The "trustees" of the NRC are the 840 plus members of the Academy. A group of that size is, of course, too unwieldy to serve as a policy-making body, even if the range of its members' opinions and prejudices are ignored. Only an estimated 225 Academy members currently serve on NRC committees, so that membership as a whole is far from perfectly informed.

The Academy council, which is elected by the membership, by and large is made up of men who combine professional distinction with a fair familiarity with the corridors of power. But the council meets for 2 days every 2 months, whereas the staff is there every day and has the civil servant's edge of a knowledge of detail.

In the last decade, the officers and council have taken steps to improve the lines of communications into the NRC and its powers of quality control. Most notably, as the broader public consequences of scientific and technical decisions became apparent, the Academy established outside NRC a Committee on Science and Public Policy (COSPUP), and the NAE was to create a parallel Committee on Public Engineering Policy (COPEP). A second article will discuss these efforts at exerting quality control and moves made toward a restructuring of NRC and also the major obstacles to change, particularly that created by the failure of the NAS and NAE to find a satisfactory modus vivendi.

Some of the problems are imposed by the congenital reliance of the NRC on part-time talent. There is a real question as to whether the increasingly complex work of the NRC can be done on the basis of gentlemanly volunteer work. Institutionally, there are also critical questions about the way committee chairmen and members are chosen and about handling of conflict-of-interest problems that arise in some areas.

Inevitably, when there is so much discussion about the various categories of contemporary "consciousness," the attitudes of an organization whose dominant majority is on the far side of the generation gap becomes a legitimate issue. Academy members are predominantly physical and life scientists devoted to their disciplines through long careers and at least mildly suspicious of the "soft sciences." They tend to be genuinely dedicated to maintaining the standards of the Academy and are appalled at the prospect of value judgments having a part in Academy studies.

Much is being made of Academy weaknesses these days, but it would be unwise to ignore its strengths. At its best, the committee system works superbly, with men of the highest competence giving disinterested advice as a public service. Unfortunately, the system works best on straightforward technical issues. And as Handler concedes, the NRC record is least impressive in the arena of the environment.

It is in this area that the greatest public sensitivity has developed. And the Academy finds itself with a new constituency—and the new experience of being judged. (Udall concluded his remarks at the AAAS meeting by urging consumer advocate Ralph Nader to conduct a study of "the Academy and the whole scientific enterprise in this country." Nader and his associates decided to undertake the project and Philip M. Boffey is leaving the *Science* news department to head the study.)

NRC was shaped in an expansionary era of American science and still reflects the spirit of that era when, in effect, it was considered as important for national scientific institutions to serve the needs of science as the needs of society. But now the Academy, like other American institutions and particularly institutions occupying monopoly positions, is having its authority questioned and is under pressure to redefine the ways in which it is to be responsive and responsible.

—JOHN WALSH

National Research Council (II): Answering the Right Questions?

If this Academy is to contribute to solution of the nation's problems, it requires easy access to those who are knowledgeable and have a kind of expertise that most members of the Academy lack. In a sense, this reflects one of the difficulties that I find in the structure of the National Research Council. The Divisions are organized along disciplinary lines: biology, chemistry, physics, behavioral sciences, engineering. But few of the problems of our society neatly pigeon-hole in the same way.—Philip Handler, in a 1969 interview when he was president-elect of the National Academy of Sciences.

Soon after Philip B. Handler took office as president of the National Academy of Sciences (NAS) in 1969 he appointed a special committee headed by Cornell's Franklin A. Long to consider changes in the National Research Council (NRC), the organization through which the NAS carries out its responsibilities to advise the government.

Critics argue that the organization of NRC along disciplinary lines limits its effectiveness in dealing with interdisciplinary problems, particularly those affecting the environment (*Science*, 16 April). Some Academy members have also felt that, as the NRC budget and staff increased, NAS members exercised inadequate control over NRC and its extensive committee operations and that some staff people indulged in uninhibited empire building.

Long says that his committee's examination of NRC structure was complicated by the unsettled status of the National Academy of Engineering, which had been established in 1964 under the NAS charter. A movement to form an Academy of Medicine was also under way, and Long says these stresses and strains inevitably influenced his committee in framing their report.

The Long committee recommendations were taken up at the NAS meeting last April and figured in a day of rather testy debate unusual for the Academy's staid business sessions.

Although phrasing its recommendations in general terms, the Long committee proposed extensive changes in the structures of both NAS and NRC. Historically, NAS has been organized along disciplinary lines in sections to which members are elected. The Long committee suggested replacing the section structure with what would be essentially subacademies of mathematical and physical sciences, life and social sciences, health sciences, and engineering. Ultimately, perhaps a fifth subunit in the behavioral and social sciences would be hived off.

NRC was to be restructured along interdisciplinary lines with major units organized to deal with major problem fields such as health, agriculture, the environment, peace, space, natural resources, and manpower.

To enable the Academy to continue serving the broad interests of the scientific enterprise, an "institute" of the Academy was proposed to deal with policy matters and with international scientific contacts and programs. Strengthening of Academy management was recommended and substantial increases in Academy membership were strongly urged, particularly in behavioral and social sciences and in health sciences. Such an increase was viewed in part as a way to prevent a diaspora of disciplines into separate academies.

Academy members had been sent a letter describing the proposed changes a month in advance of the meeting, but the Long committee's prescription proved too potent for the membership. The typical reaction seems to have been that insufficient time had been provided for reflection and discussion. The report also came up at the tag end of a day on which the members' equanimity had already been frayed. The members had again debated and resisted what has become a perennial effort by Academy member William Shockley to persuade the Academy to encourage research to establish genetic differences among racial groups (*Science*, 8 May 1970). The members had also taken up but not acted on proposals by member Richard Lewontin of the University of Chicago to alter procedures of electing NAS officers and council to open up the process. By the time the Long committee report was brought up, many members were departing to make plane connections, and there was some irritation with Handler about the timing.

The formal action taken by the membership was to "receive" the report, accept the spirit of the recommendations, and ask that a new committee carry the work further. Long says he felt after the meeting that the members' action could be taken as an act of courtesy to a hard-working committee or, on the other hand, says Long, the wording was vague enough so that an activist president and council "could do quite a lot of things." In the year that has followed, some steps have been taken on the path pointed by the Long committee but at a pace calculated not to make the members giddy. The issue of the NAS-NAE relationship remains unresolved; perhaps the major symptom is the failure of NAE to participate in a significant way in the work of NRC.

The differences between the two academies arise from a complex of causes. There have been clashes of personalities, an underlying conflict of style and outlook between scientists predominantly based in universities and engineers with industrial backgrounds and bases, friction involving status and financing between an established older organization and a fledgling newer one, and problems of NRC's prior links and loyalties to NAS. Important also was the fact that the original agreement was loosely drawn and that the hoped-for natural burgeoning of relationships did not occur. Negotiations between the two organizations are apparently at a fairly delicate stage, and both parties are being very discreet about discussing differences.

NAE president Clarence H. Linder, however, says the major outstanding issues between NAS and NAE are "how the two academies will relate as entities and how they will work in the common structure of NRC."

NAE does have basic criteria for judging attempts at reconciliation, says Linder. An NAE inside the Academy structure would have to have "high visibility." There are differences between the scientist's and engineer's approach to problems, and Linder says it is necessary that engineers "find a way to express themselves" and take leadership in their own kinds of projects. The engineers also want to be financially able to undertake some projects they feel are important without waiting for the government to come to them. Finally, says Linder, it is "very desirable to find ways to work through a reconstructed NRC."

The real problem dividing the academies, says Linder, is not structure but governance and decision making. Control of NRC is vested in the NAS council and, unless NAE is given a share in decision making, the NAE would be forced to continue to acquiesce to the scientists.

In the long interim since 1964, the NAE has to some degree gone its own way. It has developed about a dozen of its own committees. These committees report to the NAE council and are operationally independent of NRC. The prevailing feeling in NAE is that its members should be more deeply involved in committee work than are most members of NAS. The engineers have, in effect, developed their own mini-NRC but without drawing as heavily on the scientific and engineering community at large as the NRC does.

For Handler and other NAS officials, the outcome of negotiations with NAE are crucial because of the precedents that will exist if other disciplines develop separatist sentiments. The demand for an Academy of Medicine appears to have been answered satisfactorily by the creation of an Institute of Medicine within the Academy. Apparently an acceptable division of activity has been agreed on: the NRC medical sciences division will continue to deal with specific medical problems, such as those posed by drugs or shock, while the institute will concentrate on policy issues such as those affecting medical education and the delivery of medical services (see box, page 355).

Handler says he is not aware of a significant movement for a separate academy of behavioral and social sciences but says he has "the hope and strong belief" that the Academy will begin to elect a sizable number of members in the social and behavioral and medical sciences. He thinks the Institute of Medicine will provide a satisfactory solution "for at least a decade," but, in the case of the engineers, he concedes "the crystal ball is not so clear." Of the NAS-NAE talks, "It would be fair to say that those involved in the conduct of negotiations are pledged to find a modus vivendi fully satisfactory to both sides." Handler points out that NRC utilizes the services of large numbers of engineers already and that an agreement between the academies "would enlarge the responsibilities of NAE for the activities of NRC." He would be surprised, says Handler, if the calendar year ends without resolution of the question.

Meanwhile the Academy is embarked on a course of evolutionary change. In addition to the Institute of Medicine and the planned new division of transportation discussed in the article on NRC last week, Handler has built on institutional innovations made before he took office.

Perhaps the first major effort by the Academy to come to grips with the changing role and status of science was made in the early 1960's during the Academy presidency of Detlev W. Bronk. Academy member George Kistiakowsky, while serving as President Eisenhower's science adviser in the late 1950's, had grown concerned about relations between science and government, and particularly about deficiencies in planning for federal support of various fields of science. Discussions between Kistiakowsky and Bronk led to creation of the group ultimately called the Committee on Science and Public Policy (COSPUP) with Kistiakowsky as first chairman. [The origins and operations of COSPUP were described in an article in *Science*, 28 April 1967.] In its early years, COSPUP, which is made up entirely of members of the NAS, sponsored a series of studies of financial needs and scientific opportunities in various scientific fields. These studies were designed to assist federal budget planners. COSPUP also issued reports on selected important policy issues, including an influential report on population growth. Perhaps most significant, COSPUP reviewed all NRC reports with public policy implications.

In 1966 Kistiakowsky was succeeded by Harvard engineering dean Harvey Brooks (Brooks steps down as chairman in June to be replaced by chemist and Nobelist Melvin Calvin of Berkeley). During the latter half of the decade, COSPUP's relations with government altered significantly. Academy contacts generally had been with the Executive, but in the later 1960's Congress, which had paid little attention to the Academy since 1863 save for occasionally amending its charter, "rediscovered" the Academy. Mainly on the initiative of former Connecticut congressman Emilio Q. Daddario, the Academy through COSPUP began to serve an advisory role to Congress. A collection of essays titled "Basic Research and National Goals" was the first significant product, and then COSPUP developed a Daddario idea into a report on technology assessment (*Science*, 14 November 1969), which Brooks says in retrospect is the piece of work done by COSPUP during his chairmanship of which he is proudest. [NAE established a COSPUP parallel in its Committee on Public Engineering Policy (COPEP), now headed by former executive secretary of the federal marine resources council Edward Wenk. COPEP produced its own technology assessment report.]

By reviewing reports COSPUP did exercise quality control over NRC work to some extent, but a minority of reports were affected. Again Kistiakowsky, who is the Academy's elected vice-president and a sort of inspector general in spirit, collaborated with Handler in designing a new Report Review Committee (RRC), which for a year has exercised a mandate to review all NRC reports. The RRC does not play the role of censor—committees are made up of volunteers whose sensitivities are acute—but the review group does seek to assure that reports are complete, fair, clearly and concisely written, and free of conflicts of interest. RRC members are all members of the Academy, and, in view of the noninvolvement of many academicians in NRC affairs, it is revealing that fewer than five of the 80 members originally approached turned down the job. Purely technical reports are still assigned to divisions for review, but NRC committees are aware that RRC cares. Reports directed to the White House or Congress are still reviewed by COSPUP.

In addition to COSPUP and the RRC, other new mechanisms through which the NAS council exercises influence over NRC are boards and committees established outside the NRC framework. Among these are joint NAS-NAE entities, perhaps most notably the Environmental Studies Board (ESB). Created in 1967 during the presidency of Frederick Seitz who headed the Academy from 1962 to 1969, the ESB was established to oversee NRC attempts to come to grips with environmental problems which were surfacing then.

In its early period ESB activity was confined mainly to commenting on committee reports with environmental aspects, and the committee drew some unfavorable comment from critics who alleged that the group's views too strongly reflected the industry background of some of its members. Under a new chairman, Gordon J. F. MacDonald, who was last year appointed to the three-member Environmental Quality Council which advises the President, ESB adopted a more activist role. A report of the Florida jetport proposal contributed to a decision to limit the size of the airport to protect the fragile ecology of the Everglades and other neighboring areas (*Science*, 10 October 1969). Later an ESB summer study of the potential ecological effects of the extension of Kennedy International Airport runways into Jamaica Bay undergirded a decision to halt plans for extension.

The Jamaica Bay study marked a milestone, since the committee was accused by some of exceeding its charge by advising against the building of the runways. There was friction within the steering committee and among members of the ESB about the frame of reference for the study. In addition there were difficulties with the NAS council, and the NAE officials felt they hadn't been adequately informed on the progress of the study. All in all it was a major learning experience.

Perhaps the most perplexing and vexing experience arising from an environmental problem, however, came with NAS involvement in the radiation standards controversy (*Science*, 26 February). A group of federal agencies funded a major review of radiation standards by the NAS and National Council on Radiation Protection and Measurements (NCRP). NAS agreed to assess, through NRC, the biological effects of radiation on humans.

Criticism of NAS involvement in the project came from Senator Edmund S. Muskie (D–Maine), chairman of the Senate air and water pollution subcommittee, and from Senator Mike Gravel (D–Alaska). Questions about delays in undertaking the study and about the completeness of data to be studied were asked. But the main question raised was whether some members of the committee were under obligation to the Atomic Energy Commission. After the interrogation, the Academy expanded the committee and altered its composition to balance the dominance of radiologists.

NAS was further implicated in the standards controversy last year in the amendments to the Atomic Energy Act proposed by Representative Chet Holifield (D–Calif.), chairman of the Joint Atomic Energy Committee. Holifield wanted the Environmental Protection Agency to enter into contractual arrangement with NAS and NCRP to carry out a "comprehensive and continuing" study, with NAS focusing on the biological effects of radiation on man. The Administration opposed the measure, apparently successfully, on the grounds that Congress would move decision-making power out of the Executive to private organizations.

The incident poses a problem that is likely to be multiplied for the Academy since Congress has grown skeptical about placing exclusive trust in federal agencies' handling of scientific and technical issues. This is particularly true now that the Executive is controlled by Republicans and Congress by Democrats, but the doubts began long before the 1968 election. Congress looks on the Academy as a competent, independent, scientific authority, perhaps the only one around. Under these circumstances, federal agencies are even likelier to take projects to the Academy for its seal of approval.

The new congressional inclination to write NAS into legislation and to give it statutory function could create several serious problems for NAS. The Academy's option of saying no to a job could be reduced and the independence of the Academy compromised. The Academy bureaucracy would also have to be built up to handle routine business. Since the Academy is a private organization, its committees can now operate in closed session and without public records of proceedings. Insiders say this private, informal atmosphere is essential if volunteers are to continue to serve NRC willingly. If NRC had decision-making functions thrust upon it, its processes would have to be more open to public scrutiny.

Defining the mission of the Academy is difficult because its congressional charter permits such flexibility of action and the NRC is so decentralized in its operations and, in fact, it exercises such independence in accepting work. NRC policy is really defined by the contracts it accepts.

Academy critics have accused it of being a "rubber-stamp" organization by passively accepting commissions offered it by federal agencies. Handler and other Academy officials insist that work is accepted only if the job is important to the nation and nobody else can do it just as well.

Still the criteria for NRC jobs is ill defined. Some Academy members feel that the organization should tackle only narrowly defined technical questions as the only way to protect the credibility of the Academy. Those who disagree say the Academy would speedily become an anachronism since important questions have broader implications.

The current Academy attitude seems to be to exercise caution—but hardly to reject the tough questions. NRC, for example, is engaged in a study of the ecological effects of defoliant spraying in Vietnam. And under amendments to the Clean Air Act passed last year, the Academy has contracted to review the advance of auto-emissions control technology to advise on how rapidly deadlines should be imposed for reducing pollution. Kistiakowsky, a chemist, who is in close touch with the project, calls it a "hot potato" because it involves "not only technical problems but enormous economic content."

Quality control over the NRC's activities is exercised through COSPUP and the new report review committee, but this is essentially control over the final stage of the process, the "output." The NAS council, the governing body of the Academy, approves all projects at the outset, but many observers say that the bimonthly, weekend meetings with big agendas provide insufficient time for the NAS council—a group of distinguished part-timers—to be really affective gatekeeper.

After his experience as chairman of the committee scrutinizing NRC, Long says he felt that the council should concentrate on playing a policy role and be less involved in the management of the NRC. As for the administration of NRC, Long's view is that division chairmen should serve full time for terms of 2 or 3 years (two divisions are now headed by full-time chairmen—engineering and medical sciences). Long also feels that a new Academy office should be created carrying the duties of vice-president for research.

Academy management has been viewed as anything but top-heavy, since the chairmanship of NRC is combined with the Academy presidency and the offices of vice-president, home secretary, and foreign secretary are all part-time jobs. The chief administrative officer—the title is executive officer—of both NAS and NRC is John S. Coleman. Coleman is an alumnus of the staff of the NRC's undersea warfare committee and a former executive secretary of the physical science division, long regarded as the elite division of the NRC. Coleman has worked closely with Bronk, Seitz, and Handler and played a pivotal role in maintaining NRC's clubby, personal style in picking committees, hiring staff, and administering projects.

To bolster his administration, Handler brought in comptroller Aaron Rosenthal from the National Science Foundation and, as special assistant, Paul Sitton, who served in appointive posts in the last Administration and brought management experience and a knowledge of the federal system to the job. Unless structural changes are made, however, the prevailing manner of management is likely to continue.

An obvious policy issue confronting the Academy involves the frame of reference in which its committees are expected to operate. Critics have scored the Academy for its passivity, particularly on environmental issues. Former Interior Secretary Stewart L. Udall, for example, told a panel audience at the last AAAS meeting that "Whether I agree with their every conclusion or not, I admire Barry Commoner, Garrett Hardin, Kenneth Watt, Paul Ehrlich, George Wald, René Dubos, and all the others for the contribution they have made to an exciting new national debate over science, public priorities, and the future of man." Udall then went on to say that he thought the Academy had been retrograde.

Where to Draw the Line

Certainly there will be increased pressure on the Academy to take initiatives on what are being called "societal" problems. The question is where to draw the line between advice and advocacy. Academy members and officials seem acutely aware that the prestige and the credibility of the Academy depends on the degree to which the advice it gives stands up under scientific scrutiny. In a way, the disembodied conscience of the Academy is its loyalty to the scientific method. Furthermore, in a political sense the Academy membership covers a broad spectrum. As Handler put it in reply to a question of whether he thought the Academy might be "Naderized," "If we began to behave in an evangelical style, we'd no doubt be brought up short by the membership." In sum, there seems little sentiment inside the Academy for a shift from answering questions to espousing causes, but significantly many of the officials and members interviewed for this story said in almost the same words that it was important for the Academy to "answer the right questions."

The Academy style is changing, as the Jamaica Bay study testifies. When committees go beyond purely scientific and technical judgments, ways must be found to make it clear that this has been done. One study now in the works is said to carry a statement that the study reflects the views of a particular group. The matter of candor in caveats is likely to grow more important, and the whole issue of conflict of interest within committees is one the Academy will have to face squarely. NAE is said to be developing a "disclosure" rule to protect itself and its committee members.

One question that hovers over the effort at restructuring is whether the NRC should limit its efforts to work on problems of genuine national importance or should continue in the present pattern of accepting projects that range from the most important to the most routine. The Academy issues a few reports which appear to be trivial potboilers. But the controversy centers on a middle group of projects of middling value. In a sense the Academy is trapped. For an organization of volunteers to do good work it is necessary to have a good staff, and to have a good staff it is necessary to have work.

Pressure for a more selective policy is coming from within the Academy. The NAE attitude is that Academy members should be more directly involved in projects and that only projects of high national priority should be undertaken. The engineers also seem to feel that NRC is a loosely managed operation and that it could be made more responsive and efficient if a more selective policy were adopted.

Neither Handler nor the Academy council have committed themselves on this issue. Realistically, to be more selective in accepting work and to take the initiative on projects it feels are important, the Academy would require more "free money." Institutional funds available for the Academy to mount its own projects amount to only about $100,000 a year, and greater independence requires new sources of funds.

Although evolution is the operative word for the Academy and the question of NAS-NAE cohabitation remains unsettled, Academy members at their meeting next week will be faced with proposals to change the bylaws along lines laid out by the Long committee and Lewontin's suggestions for democratization of election of officers and council members. Debate is likely to be stirred by a proposal to increase the intake of new members to enlarge the membership from the present level of about 850 to some 1200 over the next several years, with the increases concentrated in the social and behavioral sciences and medical fields.

The Academy has been moving from an almost exclusive concern with the relation of science to government to consider also the relation of science to society. Its critics say it is not moving fast enough.

Doubtless the Academy, however reformed and restructured, will continue to serve the interests of science and to serve government, but it is unlikely, in the future, to define the public interest simply in terms of the requests for advice from contracting agencies.

—John Walsh

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Federal Support of Research Careers

Government joins universities to increase the number of career appointments in research.

James A. Shannon and Charles V. Kidd

For some years it has been evident to qualified observers that the absence of adequate numbers of stable career opportunities for scientists has been an increasingly important barrier to the establishment of a sound research structure for the nation as a whole.

During and since World War II, university research in the United States has been heavily dependent for support upon federal grants and contracts. This support is often, although not always, provided for long periods of time. In many fields of science, support for research has grown at a pace exceeding the capacity of universities to staff the programs from their regular sources of income. The staffing problem has been solved in various ways. Large research organizations have been set up outside universities. Government laboratories have been expanded. Finally, universities have adopted practices enabling them to undertake larger research programs without committing a correspondingly larger proportion of university funds. These practices have included such steps as payment of the salaries of faculty members and other professional people from research grant and contract funds.

Increasing numbers of investigators, particularly at the assistant and associate professor levels, receive all or a large part of their income from research grants and contracts. This situation arises not from reluctance to pay staffs from stable funds, or from misgivings as to the quality of the group whose salaries are derived from grants and contracts. The research programs of the nation have simply expanded more rapidly than the financial base of stable funds available to universities. This development has been necessary to expansion of research in universities, but it has had some unfortunate consequences. First, the number of investigators whose salaries are dependent upon renewable research grant or contract support has now become so large as to create an unhealthy degree of uncertainty as a built-in characteristic of the system. Second, many of the individuals concerned, and their families, lead a sort of hand-to-mouth—or grant-to-grant—existence. This is not conducive to the best work, nor is it an equitable arrangement. Third, the salary arrangements have tended in academic institutions to be a devisive force, by creating a group of scientists who have few—and in many cases no—teaching responsibilities. Finally, the system does not provide an adequate investment in the future research capacity of the nation by strengthening the teaching process to the optimum degree.

The Public Health Service, with the approval of the Congress, is in the process of initiating a program aimed directly at the solution of these problems in the fields of medical and related research. This article deals with the development of this program for increasing the stability of research careers in medical research through the grant program of the National Institutes of Health. In this presentation, in addition to defining the principles of the operation of this new program, we discuss some of the problems which have arisen during the early stages of its implementation. Most of these stem from new relations that are emerging between the federal research support programs and institutions of higher learning.

In essence, this is a case study of the problems which arise when the federal government supports research in universities which have responsibilities extending beyond research to teaching. If the federal government looks to the research capacity of the nation 10 or 20 years in the future, as well as its current research capacity, it must be concerned with the ability of the people who will be investigators in the coming

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decade and beyond. Those people are now students, and they are being taught in universities. Accordingly, if the federal government's concern for medical and related research is to be a continuing concern, it must take into account the training of investigators for the future, as well as the support of scientists who are now fully trained. This transition from support of medical research in a narrow sense to support of the full structure and range of activities necessary to provide a sound scientific program in medicine and the related sciences for the indefinite future is the central problem of federal research policy in this area of research support. The problems involved in establishing a sound program for supporting careers in research are a specific aspect of the more general problem.

The Committee of Consultants on Medical Research to the Committee on Appropriations of the United States Senate issued in May 1960 a report, "Federal Support of Medical Research," which recommended that: "Funds should be provided through the National Institutes of Health in fiscal year 1961 to support the establishment of 200 research professorships in medical and dental schools and the basic science departments of colleges and universities at a salary level of $20,000 a year, the funds to be made available to, and administered by, the respective institutions."

Subsequently, $2 million was appropriated for this purpose, and an announcement of the plan to establish a "research professorship award" was made. The guides for administering this program, although issued at that time, were subsequently withdrawn and revised. The original guides were as follows.

1) Schools could nominate full professors (with provision for nomination of associate professors in unusual circumstances.)

2) Awards would be competitive, and selection would be based upon the distinction of the nominee. In the words of the brochure announcing the program, "Career Research Professors will be selected for support on the basis of demonstrated capacity to pursue with distinction a professorial career in independent research and teaching," in the fields of medicine, dentistry, public health, and related sciences.

3) Schools could nominate persons with tenure, and with income derived from stable sources, provided they agreed to use the released money for other professional positions in the school.

4) Awards would be for 5 years and would be renewable.

These guides were discussed with a large number of individuals from universities and with representatives of professional organizations. At that time, questions raised by these individuals related chiefly to such matters as the eligibility of faculty members with administrative duties, the firmness of the federal commitment to provide stable support, the fate of the award if the awardee changed schools, and other matters largely of an administrative nature.

Problems with the Original Program

Over the ensuing months, as institutions selected candidates under these initial guidelines, some more fundamental questions arose in the minds of persons in the institutions and in the federal government. These were as follows.

1) Some institutions had begun to have misgivings over the terms of a program which involved the federal government, even indirectly, in the selection of professors.

2) Some institutions were reluctant to make nominations because they did not wish to place full professors in national competition with each other or with professors from other institutions.

3) Other institutions were reluctant to make nominations because they felt that the award would not be stable, basing this view upon the words of the guide indicating that the awards would be initially for 5 years with a promise of support for the additional years, contingent upon annual appropriations.

4) On the part of the federal government, it was realized, as applications were received, that there had not been an adequate understanding with the universities as to the nature of the commitments that both they and the federal government should assume if a program for career support were to be fully acceptable and productive, and of the qualifications of candidates.

5) When the applications were reviewed *as a group*, it was found that a high proportion of the applicants were full professors of high distinction who were approaching, or who had entered, the final stages of their careers. Each institution acted in good faith within the terms of the guides, but the group as a whole did not possess characteristics in accord with the intent of Congress.

6) It was also found that a high proportion of the nominees were full professors with tenure. The effect of the program would have been largely to release the university funds formerly used for the payment of the salaries of full professors for the appointment of junior faculty. This was not the intended major effect of the program, as described to and accepted by the Congress. Thus, the desired objective of providing greater opportunities for stable career support for individuals paid from grant funds would have become a secondary and purely fortuitous result of the program.

No one of these reservations was conclusive. Some of them were mutually exclusive, and they were given various weights by those concerned with the program. But in total, they constituted substantial reason to review with care both the fundamentals and the operating guides for the program. Such considerations led the National Advisory Health Council, a group of citizens advisory to the Public Health Service, to pass a resolution on 15 March 1961 which recommended "That the Career Research Professorship Program, in its present form, be abandoned before implementation in the form of specific awards."

In the light of this resolution and of further consideration of the applications and the guides, a Committee on Career Research Professorships, which had been convened to make recommendations on applications, was asked to consider the basic elements of the program and to advise on its future.

On the basis of the considerations brought forward by this group, and of further deliberation, it was decided (i) not to make awards under the existing guides; (ii) to return the appropriated $2 million to the Treasury; (iii) to revise and issue new guides as quickly as possible; (iv) to return all applications with a request that institutions review them in the light of new guides and make new nominations; and (v) to make awards in the second half of calendar year 1961.

Policy Questions Clarified

Experience with the original guides sharpened some points of policy which had hitherto not been clearly stated.

1) A program designed to provide stable career opportunities for the large group of capable investigators receiving support from unstable sources—largely grants and contracts—could not simul-

taneously serve effectively to provide awards to career research professors who would be selected for support on the basis of demonstrated capacity to pursue with distinction a professorial career. The program had to be designed to do one or the other.

2) A program designed to increase the number of scientists supported from stable funds could not be based on standards encouraging nomination of persons with assured positions and assured income. The program would have to be designed primarily for those whose incomes were derived from unstable sources.

3) There should be no possibility that the inference might be drawn from the program guides that the federal government was selecting professors.

4) If the program were to provide a source of income more stable than that provided by research grants, the federal government would have to make awards with the firm intention of continuing them for the productive careers of those selected. Awards in segments of 5 years, renewable upon review at the end of each 5-year period, would not provide the necessary stability.

5) Whether the program was designed solely for persons engaged full time in research and teaching, or whether those engaged in research and teaching on a part-time basis would be eligible, was a question to be decided. Furthermore, if only full-time persons were to be eligible, "full time" would have to be defined. The original guides were silent on this point.

6) A federal program designed to provide stable career opportunities in academic and other research environments involves a long-term relationship between the institution and the federal government. The institutions, as well as the federal government, must assume appropriate responsibilities for the career stability of those given federal awards. This question was not dealt with in the earlier guides.

The points enumerated above seem in retrospect to be the kind of conclusion that one would reach by quiet and fairly brief reflection. The fact that many experienced people from universities, foundations, independent research organizations, and the federal government did not reach these conclusions in the initial discussions bespeaks the complexity of the problems that arise when a new policy affecting long-range government-university relations is adopted by a federal agency, and particularly when answers must be stated quickly.

New Guides

The re-examination of the premises underlying the original program, and of the specific guides for the program, led to the development of a new program concept and a new set of guides. The program has been designated the "NIH Research Career Award Program." In summary, the revised program, which will go into effect during the federal fiscal year that began 1 July 1961, has the following characteristics.

1) The primacy of the intent to increase the number of stably financed academic and other research positions is established. Accordingly, candidates whose salaries are derived primarily from research grants or contracts and from similar sources of relatively short assured duration will be given preference.

2) Conversely, the objective of providing awards designed to recognize outstanding scientific excellence, and to provide status and prestige to the individual and his institution, is subordinate. The awards will be competitive, and the standards for awards will be high, but the area of competition will be primarily among those whose incomes are from sources of relatively short assured duration.

3) To provide a system for support of research careers, it is necessary to distinguish between various levels of career development, because the needs of individuals and institutions vary at different levels. Accordingly, two groups of awards have been established. "Research career development awards" are designed for those who are in the early years of research careers. To be eligible, candidates must have had at least 3 years of relevant research or professional experience after receiving the doctorate. Awards are for 5 years, renewable, upon adequate justification, for 5 additional years. "Research career awards" are designed for those with substantial experience who are already launched upon research careers; these awards will provide support for the full career of the individuals who are selected.

4) An important objective of the program is to strengthen research institutions, while providing support to individuals. To provide a continuing link between the individuals selected and their institutions, a number of ties to the institution are preserved under the program. Awardees are expected to participate in the general activities of the institution, including teaching. Awards are not made to individuals but are made to institutions on behalf of individuals. The NIH award will be consistent with the salary scale of the institution for persons with comparable experience and accomplishments. Finally, the institution is asked to nominate for "career awards" only those whom it would wish to have as permanent staff. Taken together, these provisions should link the institution and the awardees effectively under a program which provides salaries from a federal agency.

5) The awards are intended to provide sufficient compensation to permit those who are selected to devote their careers to research and teaching. Consistent with the principle that awardees are intended to be integral members of faculties, or of research staffs of nonacademic institutions, the award will correspond to the salary paid by the institution to other persons with comparable attainments. Since the object of the program is to free people for careers in research and teaching, those who receive awards will be expected to devote themselves full time to these activities. However, the award recipients will be expected to engage in the usual ancillary activities of faculty members, such as writing, delivering occasional outside lectures, and serving on advisory groups, and they may receive the usual compensation for such work. Awardees will also be expected to practice their professions, as may be indicated, in connection with their teaching and research duties, and in order to maintain their professional skills. However, they may not retain personal income from practice.

Scope of the Program and Its Future

The program has been devised to meet very clearly defined and limited objectives. Accordingly, many individuals of high competence will not be eligible, and many institutions may find that they either cannot or do not wish to nominate persons for awards. For example, some medical schools which permit their faculty members to retain substantial sums from the practice of medicine may feel that they prefer their present system and do not wish to make the changes required to make faculty members eligible.

The fact that candidates with unstable incomes will be given preference, and the concurrent administrative intent to sustain high standards of excellence, will tend to concentrate awards below the senior academic levels. This is the case because the prevailing practice is to give first priority in the use of firm institutional funds to the payment of salaries to the most able senior faculty members. If the needs for firm funds for payment of salaries to outstanding persons progressing to senior positions expand more rapidly than the firm institutional funds available for salaries, the federal funds for career support will become progressively more important at the senior levels.

In terms of money, $4 million is available in the year that began 1 July 1961 for the Research Career Award Program. It is anticipated that this will finance about 275 awards.

As a long-range possibility, amalgamation of parts or all of this program with the new General Research Support Grant program will be considered. The General Research Support Grant provides broad aid for medical and related research, not support in the form of aid to specified projects or programs. The General Research Support Grant is a single grant to an institution, allowing it to meet those direct costs of research not covered by other forms of research support which are, in the judgment of the institution, most urgent. For these grants, $20 million will be available in calendar year 1962 to schools of medicine, dentistry, osteopathy, and public health. The grant will be increased and extended to other institutions engaged in medical and related research in subsequent years.

To view federal support for research in universities in perspective, the Research Career Award Program represents a shift towards emphasis upon the long-term support of highly qualified people for research and teaching, as constrasted with support of current research. The General Research Support Grant represents a trend, evident in the actions of a number of federal agencies and most explicitly in the institutional grant of the National Science Foundation, toward aid to research and education on a broad basis, detailed determinations being left to the institutions. Accordingly, the long-range relationships between the programs must be taken into account in considering the evolution of the grant programs of the National Institutes of Health.

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Medical Research: Past Support, Future Directions

Aims of the National Institutes of Health are surveyed as its annual budget passes the half-billion mark.

Dale R. Lindsay and Ernest M. Allen

The health status of the nation is a complex matter, involving many factors. Cancer, tuberculosis, heart disease, pneumonia and influenza, arthritis, blindness, deafness, mental illnesses, diabetes—these are only a few of the hundreds of diseases and disabilities that have long afflicted mankind and that still persist as greater or lesser health problems in this and other countries.

New diseases have appeared in the world from time to time, and the industrial age has brought with it environmental health problems not dreamed of by earlier generations. Left to themselves these influences, together with the greater opportunities for the spread of contagion in a crowded urban society, would have brought our national health level to a new low, beneath that of the preponderantly rural society of a century ago. Yet, as we are all aware, such have been the advances in the broad attack upon these influences that there has been a steady improvement in the health status of the nation.

The picture has not been one of uniform improvement on all fronts, as may be seen in the death rates for our two major killers, heart disease and cancer (Table 1). We find encouragement, on the other hand, in figures such as those in Table 2, for three other disease categories. Still other diseases have declined to so low a level of importance in the total health picture that they must be looked for only among the fine details. Typhoid fever, malaria, and smallpox, once scourges, have been tamed. The hookworm problem is steadily diminishing in importance in areas where hookworm was once so prevalent. Pellagra is almost a thing of the past.

Health Parameters

We may feel the need of an over-all measurement that expresses or reflects the nation's present health status and permits us to evaluate past and future change. One that is informative is the age-adjusted death rate in our population for deaths from all causes. It stands now at only 44 percent of the death rate at the beginning of the century and has gone down appreciably even in the past several years (Table 3).

Another over-all measurement, a different health parameter of the population, is the average life span, known technically as the "life expectancy at birth." It stands at the highest figure in our history, is among the highest in the world, and has risen noticeably in even so short a period as the past 8 or 9 years (Table 4).

Further information, of a different sort, dealing with the prevalence of *all* illnesses, not just those that have a fatal outcome, might be had from figures on the average number of days per person per year lost from work or other normal activity because of illness—the average days of "incapacity." No information from which to compute this additional parameter is available for the past decade, but we may anticipate that such data for coming years will be available in the future (*1*).

The death rate, average life span, and average days of incapacity are not, of course, the only informative parameters of the health of a population that one might desire. The summary data that are available and that are given here, however, do reflect the generally favorable trend observed in the past half century and more. They also bring to sharp focus a challenge: It is necessary that the trend, where favorable, be continued or even accelerated, and that every effort be made to reverse the present trend in the incidence and outcome of diseases, such as heart disease and cancer, which have not yet responded favorably.

To accept such a challenge, it is necessary to understand the factors responsible for the improvement in health

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Table 1. Age-adjusted death rates for the continental United States, exclusive of Alaska, for heart disease and cancer.

Year	Deaths per 100,000 population	
	Heart disease	Cancer and other malignancies*
1900	167	80
1950	300	126
1959	291	127

* This category includes leukemia.

that has taken place. One factor is surely the higher economic level of our society: A rising per capita income has made possible a larger investment in health measures, both by the individual and by philanthropic agencies and the state. We shall not attempt to evaluate the magnitude of the contribution of this factor. It may be large, and in the short view it may be even larger than that of the other major factor, research. In short, most or much of the improvement could conceivably be a "catching up," a putting to good use the research findings of the past.

Certain it is, however, that for the improvement to continue, research and ever more research will be necessary. Without it the upward progress in health would necessarily level off to a plateau.

The National Institutes of Health has played a significant part in the support of medical research for only a decade and a half, but its role seems destined to be of even greater significance in the decade to come. In view of the growing importance of NIH on the biomedical research scene, it seems fitting to present a brief account of the growth of research support provided through NIH in the past, of what has been achieved, and of what seems to lie ahead.

Historical Background

At the beginning of the century the part played by the federal government in the drama of medical research was small indeed. Philanthropic granting agencies and universities, with some participation by industry, together with private individuals, constituted the main sources of research support. The federal government's participation traces back only to 1887, when a one-room laboratory, a "laboratory of hygiene" devoted to bacteriological studies on returned seamen (studies of cholera, tuberculosis, typhoid fever, diphtheria, and so on), was established in the Staten Island Marine Hospital. In 1891 this laboratory was moved to Washington, and in 1905, with a greatly expanded research mission—that of investigating "infectious and contagious diseases and *matters pertaining to the public health*"—it moved into its new laboratory building at 25th and E Streets, adjoining the Naval Hospital. It had come to be known officially, in the meantime, as the "Hygienic Laboratory." The research areas into which it extended its activities further increased in number thereafter; cancer was included in 1922, and mental hygiene in 1930. In the latter year the laboratory, with its several divisions, was rechristened the National Institute of Health—a name which was changed in 1948 to the present National Institutes of Health.

It was not until 1938 that the federal medical research effort expanded beyond the confines of government-operated laboratories. The expansion was through grants-in-aid to universities and other private research institutions under the newly inaugurated "research grants program." In that fiscal year the effective appropriation was $91,000. In the next several years, ending with 1945, appropriations for research grants remained at or below this level, but in 1946 an "*expanded* research grants program" came into being, with $780,000 in funds. The next year (1947) the program experienced an increase that was spectacular for the time, to $3.4 million, and now, 14 years later (fiscal year 1961), it stands at $287 million. The "intramural" research effort, within the confines of the National Institutes of Health, grew in the same period from $2.3 million in fiscal year 1946 to $98.4 million in 1961.

The figures for the successive years may be seen in Table 5.

It is important to note that, in the same period, *non*federal support of medical and related biological research also underwent a very substantial increase (for example, from $60,000,000 in fiscal year 1947 to $333,000,000 in 1960). Clearly, the great expansion of federal support has by no means acted to dry up nonfederal funds; it is reasonable to believe, on the contrary, that the increased harvest of research accomplishments brought about by the federal outlay has actually stimulated support of medical research through voluntary channels. Certainly both federal and nonfederal support have risen, each at an unprecedented rate, in the last 15 years, and particularly in the last decade.

Research Achievements

What has been achieved with this unprecedented outlay of federal and private funds? In the first place, to name an intangible but important achievement, there has been a great expansion and intensification of public interest in medical and related biological research. New research findings, if they have news value, are likely to be reported to the general public by the science writers for our newspapers and other periodicals. The average citizen is, accordingly, better informed on health matters than ever before, more "research-minded," more aware of the hopes that research can offer, more insistent that we "get on" with the task of research toward beneficent ends. With this growth in alertness to the promise and importance of medical research has come a willingness to contribute to its support—a willingness to have a greater share of the tax dollar invested in medical research and a willingness to make additional contribution to this urgently necessary activity through nonfederal channels. This aroused interest and willingness to contribute must be regarded as a major achievement of the greatly expanded research effort that has come about, under federal leadership, in the last decade and a half, and particularly in the past 10 years.

Basic research. A second result of the developing research-mindedness of the American public is the greater public understanding of the essential part played by basic research in our effort to conquer disease. Basic research may be likened to the submerged part of an iceberg: It does not call attention to itself, but it provides indispensable support for all applied research directed toward the control or conquest of disease.

NIH-supported research. Although both federal and nonfederal funds for medical research are fundamentally from the same source, differing only in route, it may seem important to attempt to give "credit" to the National Institutes of Health (as to other federal agencies) for the research accomplishments resulting from its grants to universities and other research centers and from the research conducted within this great medical center itself. It is easy enough to enumerate some of the more important discoveries in the medical and related biological sciences that have been made in the past decade or so, and to list specifically some that have been made in the course of work supported by research grants from the

Table 2. Age-adjusted death rates for the continental United States, exclusive of Alaska, for three disease categories.

Year	Deaths per 100,000 population		
	Tuberculosis	Influenza, pneumonia*	Gastro-intestinal inflammatory disease†
1900	199	210	113
1950	15	24	4
1959	6	24	4

* Exclusive of pneumonia in the newborn.
† Exclusive of the newborn.

NIH. This is done elsewhere in this article. It is important, however, first to understand federal support as it has influenced the total body of medical research, regardless of the source of support.

Research expenditures by the National Institutes of Health in fiscal year 1950 represented approximately 18 percent of the total national outlay for medical research. For fiscal year 1960, the percentage stands at an estimated 40 percent, or double the earlier figure. An average of the two, expressed roughly as "one out of three," may be taken as representing the entire 10-year period. One out of every three dollars spent for medical research in the decade was spent via the NIH. Certainly during the latter part of the decade, it may be presumed, one out of every three research findings—big or little, basic or applied—came to light during work financed through NIH support.

But the other two out of three research findings were not isolated—quarantined as it were—from scientific contact with the one. On the contrary, each of these, and indeed every research finding, owes something to other findings that have preceded it. A recent quick count of the number of bibliographic references appended to each of five papers in ten representative journals in the medical sciences reveals that one scientific paper refers, on the average, to between 25 and 30 previously published papers. These papers have contributed either to the investigator's conception of the problem he has attacked, or to his method of attack, or to his interpretation of his findings, or to all three. His work, like the capstone of a pyramid, rests upon the work of others. He has seen findings reported in recent issues of journals, heard them reported at scientific meetings, and even learned about new findings in private conversation with fellow scientists in the same general field of interest.

Table 3. Age-adjusted death rates for the continental United States, exclusive of Alaska, for deaths from all causes.

Year	Deaths per 100,000 population
1900	1778
1950	840
1959	770

In short, this "one recent research finding, out of every three," that may conservatively be attributed directly to NIH support has itself undoubtedly been an essential step leading toward the other two, or to *some* other two in the total body of advances in medical research. To disentangle the research achievements clearly creditable to NIH support from the achievements to which NIH support has contributed indirectly by making them possible as "a next step," is quite an artificial separation. To use a phrase from Scripture, "the little leaven leaveneth the whole lump" and cannot thereafter be extracted from it. It must be said that in recent years NIH-supported research has been an important factor, has played an inextricable part, in the general advance in knowledge and practice toward the control of disease, in every area in the total field of medicine and related biology.

The picture would be distorted if the presumably even greater influence *on* NIH-supported research arising from research supported by other agencies—60 percent of the national total, as estimated for 1960—were not also pointed out.

Number of projects supported. The influence of NIH-supported work has surely permeated the whole body of modern research in the medical and related biological sciences, but is it possible to sharpen the focus a bit? Can one be more specific about what the nation has got for the tax money channeled through the NIH? How many research projects have been supported? How many papers have been published? What of importance has been discovered?

The number of separate research "projects" given NIH support in each of the years in the past decade may be seen in Table 6, column 6. The average "dollar size" of a project (col. 2) can also be computed for each year, by dividing the total amount of funds granted by the number of projects. It is recognized, of course, that there is a wide spread in the annual dollar size of individual projects supported by the NIH. Some cost less than $1000 for 1 year; others cost more than 100 times as much. They also vary correspondingly in personnel and equipment, ranging from a single investigator with his microscope to half a dozen interdisciplinary teams, each working with complicated and costly facilities, in half a dozen scattered research centers.

Table 4. Average life expectancy at birth (age-adjusted rates for the continental United States, exclusive of Alaska).

Year	Life expectancy at birth (yr)		
	Total population	Males	Females
1900	47.3	46.3	48.3
1950	68.2	65.6	71.1
1959	69.7	66.5	73.0

The number of NIH-supported research projects can be appreciated better when it is viewed as a component of the estimated total number of medical and related biological research projects in the nation (Table 6, col. 3). These estimates have been computed by dividing the estimated total medical research expenditure (*2*) for the nation by the "average dollar size" of an NIH-supported research project—that is, the average dollar outlay per year per project (col. 2). The estimates in columns 4 to 6 have been similarly computed. Underlying these computations is, of course, the assumption that the average dollar size of a project, in NIH experience, can be used as an estimate of the average for medical research in general (see Table 6, footnote *).

The NIH supported 10,700 projects in universities and other research institutions in 1960, out of a national total estimated at 38,500 projects.

Table 5. Funds for NIH-supported medical and related biological research for fiscal years 1946 through 1961.

Fiscal year	NIH-supported research ($, millions)		
	Extramural*	Intramural†	Totals
1946	.8	2.3	3.1
1947	3.4	5.0	8.4
1948	8.9	7.5	16.4
1949	10.9	10.3	21.2
1950	13.1	12.7	25.8
1951	15.6	14.8	30.4
1952	18.2	13.9	32.1
1953	20.3	17.9	38.2
1954	28.9	19.9	48.8
1955	33.9	24.9	58.8
1956	38.6	32.4	71.0
1957	80.6	44.6	125.2
1958	100.0	57.4	157.4
1959	140.7	68.9	209.6
1960	199.2	84.6	283.8
1961	286.9	98.4	385.3
Totals	1000.0	515.5	1515.5

* Grants. † Includes field investigations and administration of research and research grants.

Table 6. Average size of projects (in dollars) and number of medical and related biological research projects for fiscal years 1950 through 1960.

Fiscal year	Average* size of project ($)	Active medical research projects (No.)			
		Throughout the nation†	All nonfederal†	Federal (including NIH)†	NIH extramural research‡
1950	9,649	15,300	9,100	6,200	1,400
1951	10,601	15,400	8,500	6,900	1,500
1952	10,658	16,200	8,800	7,400	1,700
1953	10,261	19,800	10,400	9,400	2,000
1954	11,203	20,100	10,500	9,600	2,600
1955	11,379	21,100	10,700	10,400	3,000
1956	12,470	22,900	12,000	10,900	3,100
1957	14,209	28,000	14,900	13,100	5,700
1958	15,300	32,100	17,300	14,800	6,500
1959	16,584	35,400	17,900	17,500	8,500
1960	18,584	38,500	18,000	20,500	10,700

* "Average" means the average for all projects supported by NIH research grants. These averages were used as estimates of the national average in calculating entries in columns 4 to 6. For the separate institutes, averages for fiscal year 1960 were as follows: Arthritis and Metabolic Diseases, $16,200; Neurological Diseases and Blindness, $21,700; Cancer, $21,100; Dental Research, $13,500; Allergy and Infectious Diseases, $15,000; General Medical Sciences, $18,200; Heart, $19,300; Mental Health, $21,600. The NIH averages in column 2 may possibly be somewhat higher than averages for the nation. † Estimates, calculated by dividing the figures for total reported research outlay (not shown) by the amounts in column 2. Discrepancies are due to rounding of figures. ‡ NIH research grants program.

Number of papers. On publication of a paper from NIH-supported research, the author is asked to (and usually does) supply a reprint for the NIH files. These files are, unfortunately, incomplete prior to 1957; the count for 1957 and later years (believed to be 90-percent complete) is shown in Table 7.

A backward extrapolation of these figures is quite speculative but suggests that the number of papers from NIH-supported research for the year 1950 was in the neighborhood of 2000. The total for the 11 years ending with 1960 is conservatively estimated to be 50,000 or more.

Each paper reports from one to several research findings in its field.

There is reason to believe that the findings from NIH-supported research are of somewhat greater than average scientific importance, for, although the judgment of the mature and experienced investigator of scientific standing is, and should continue to be, sufficient certification of the importance—indeed, the necessity—of any research he proposes, every project supported by an NIH research grant has, nevertheless, been in a sense doubly certified as to its scientific importance and necessity (*3*). Each project awarded an NIH grant has been endorsed by a "jury" of from 10 to 20 distinguished scientists who have studied the proposal, and has been given further consideration by an advisory council of equally distinguished members and recommended by them to the surgeon general of the Public Health Service for grant-in-aid support.

It is reasonable to believe that research undertakings that have been so competently scrutinized and screened constitute, as a body, an aggregate research effort of superior worth and promise. Even if the results from NIH-supported research were not identifiable as such in the vast output of the nation's research laboratories and only the total forward march of research achievement could be perceived, it could still be said with assurance that most of the work coming out of the laboratories receiving NIH support (together with research supported by other agencies using similarly effective screening mechanisms) must be in or near the forefront of the procession.

Listed below are a few research findings from the thousands of significant advances in medicine and related biology that have been made in the past decade in the course of research supported by the NIH (*4*). It should be pointed out again that such findings are but capstones of "pyramids" of findings by many workers, supported by many agencies. These peak findings will, of course, be built in turn into the lower levels of other such pyramids, to be capped by further achievements.

Some Research Findings

Prednisone, a synthetic relative of the steroid cortisone, was found to be as effective as cortisone or hydrocortisone, or more so, for treating rheumatoid arthritis, and to cause less edema or none at all.

The folic acid antagonist methotrexate has been found to have pronounced beneficial effects in cases of choriocarcinoma, a variety of cancer.

With the albino hamster as the experimental animal, it has been shown that dental caries can be both infectious and transmissible. The organism is a streptococcus. A different study has shown that fluoride (1 part per million) in drinking water has a dramatic preventive action in children.

The hemadsorption viruses, members of the parainfluenza group, were isolated and shown to cause many of the acute respiratory infections in children, from afebrile infections to such conditions as croup and pneumonia.

It has been shown that giving cod-liver oil to pregnant rats reduces the incidence and severity of congenital anomalies caused by deficiency of vitamin E in their diet. A change in the balance of the remaining vitamins in the diet apparently compensates to some extent for deficiency of the single vitamin.

The adrenocorticotrophic hormone (ACTH), a protein hormone containing 23 amino acids, was synthesized from the natural amino acids. This is the largest protein molecule yet synthesized.

Convincing evidence that the onset of acute multiple sclerosis, in a case of the disease in man, resulted from injection of rabies vaccine (containing elements of nervous tissue) has strengthened the view that multiple sclerosis, as it ordinarily occurs, is an autoallergic disease representing an immunologic response to some unknown chemical constituent of the patient's own nervous tissue.

Raising the brain's concentration of gamma-aminobutyric acid (GABA), a normal constituent of the brain, has been found to give protection against convulsive seizures.

Scientists studying epilepsy were handicapped by their inability to reproduce it in any laboratory animal until it was found that, after a simple surgical operation in which alumina cream is applied to a very small area of the brain surface, the experimental animal for some months becomes an "epileptic," having typical epileptic attacks.

A viral agent associated with mouse leukemia has been found to acquire such potency in serial passage in tissue culture that it can produce *multiple* primary tumors in mice and sarcomas in hamsters and rats. This discovery strengthens the view that viruses may be at least one of the causes of cancer in man.

Chloroquine and pyrimethamine were

shown to be suppressive, and primaquine was shown to be curative, of malaria. These drugs have now been adopted for use in the U.S. military forces.

It has been shown that the placenta will, if necessary, deplete levels of vitamins B_{12}, B_6, C, and iron in the mother's blood in maintaining these nutrients at more nearly normal levels in the fetal blood stream.

It has been shown that forced oral (or intravenous) administration of large quantities of a solution of one teaspoon of table salt and one-half teaspoon of baking soda in a quart of water can serve in cases of burn shock as an emergency substitute for plasma. In another study it has been shown that, of individuals treated with balanced salt solution and individuals treated with whole blood, more of the former survive.

A living virus, the "tobacco mosaic virus," has been taken apart, into its skeleton of ribonucleic acid and the latter's protein envelope, and reconstituted. The ribonucleic acid is the primary source of the infectious activity of the virus. These findings will lead to a better understanding of the pathogenicity of viruses.

Two specific tests, each based on the bentonite flocculation procedure, now permit diagnosis, in a few minutes, of rheumatoid arthritis and lupus erythematosus.

Mapping of the gene locations in the chromosomes of the red bread mold *Neurospora* is continuing and will contribute information that will ultimately be useful in the effort to unravel the mystery of the action of deoxyribonucleic acid and ribonucleic acid as code-determinants of the hereditary structure and function of all organisms.

It has been found that galactosemia is due to the absence of the enzyme P-Gal transferase—a genetic defect. A quick test on the blood permits diagnosis and prompt institution of a galactose-free diet.

A culture medium of chemically defined composition has been developed that has made it possible to maintain cultures of cells from a variety of tissues (such as normal skin, bone, kidney, connective tissue, and cancers) indefinitely.

It has been shown that production of the fetal type of hemoglobin is favored by oxygen and glucose deficiencies.

A new drug, phenazocine, first conceived on the "drawing board" and then synthesized, has been found to be many times more potent than morphine in relieving pain.

Table 7. Number of reprints of papers on NIH-supported research (extra- and intramural) in the files of the National Institutes of Health.

Year	Number of papers
1957	5,230
1958	5,895
1959	8,364
1960	11,000

An erythropoietic factor is formed by goats exposed to a simulated altitude of 22,000 feet and is secreted in the milk. Injected into rats, it raises the blood hemoglobin and the reticulocyte count.

A new pathway for sugar metabolism, the "hexosemonophosphate shunt," has been discovered. It bypasses the citric acid cycle and contains several previously unknown sugars.

Evidence has been obtained that liver changes similar to those in carbon tetrachloride poisoning can be brought about by central stimulation of the sympathetic nervous system.

Two independent investigators have won Nobel prizes for nucleic acid synthesis—the one for discovering an enzyme that synthesizes deoxyribonucleic acid, the other for discovering an enzyme that synthesizes ribonucleic acid. These two nucleic acids code-control bodily structure and function, apparently throughout all animal and plant life.

A molded plastic replica of a normal mitral valve of the heart has been constructed and used successfully to replace a diseased valve in man.

This list could be greatly extended.

Research achievements summarized. We might summarize research achievements through NIH support as follows.

1) In 1950 the number of published papers from NIH-supported research projects appears to have been in the neighborhood of 2000 or 2500 (reliable figures are not available); in 1960 the number is reliably estimated at 11,000. The total number of such papers from (and including) 1950 through 1960 is conservatively estimated at 50,000.

2) In 1950 the number of active research projects receiving NIH support through its program of research grants to medical schools and other institutions conducting medical research amounted to 1400, out of a national total of medical research projects estimated at 15,000; in 1960 the number had grown to 10,700, out of a national total estimated at over 38,000.

There is every reason to believe that this NIH-supported research (and research supported by other granting agencies with similarly effective screening mechanisms) has, on the average, been of superior scientific merit and importance.

3) The number of research findings reported in papers published in the period 1950–60 that give credit to NIH for support cannot be estimated. At a minimum it would be expected to equal the number of research papers published in the period (estimated at 50,000) and might well be two or three times that number.

4) NIH-supported research has made an inextricable contribution to the total progress of medical science and its achievements in the last 10 or 15 years. The past 10 years' research supported by this and other granting agencies active in the medical and health research field has, without much doubt, played a part in the fall in death rate and the rise in life expectancy that have occurred even in the same decade. It is reasonable to expect that continued or expanded biomedical research in the next decade will have an increasingly important impact on the health of the nation.

Future Research Opportunities

Let us now look beyond the periphery of present biomedical knowledge and mention a few of the areas where it appears that intensive exploration would be rewarding.

It should be understood that no attempt at complete coverage of research opportunities will be made here, and no attempt to shape the pattern of the discussion into conformity with any preexisting formulation, such as the balanced pattern of program interests of the several institutes of the NIH. An attempt will be made, however, to convey the restrained enthusiasm of many of the group of competent scientists who have left their laboratories in order to render a broader service to medical science through their office in the Division of Research Grants of NIH (5).

Instrumentation. The objectives of instrumentation and automation research have been succinctly stated as follows: "to measure (and record) more things, more accurately, and automatically."

It has long been said in science that the ability to measure some important quantity with greater precision by one more decimal place opens up a new era of advance in the scientific field. To be conservative, one might amend the saying to read "two more decimal places" —measurements 100 times more precise. The core of truth in the saying is that the progress of biological science is ultimately dependent upon development of ever more sensitive instruments and methods for making ever more precise measurement of an ever wider variety of things, and that an explosion of new research follows a new and important development in instrumentation. A modern example is the burst of research progress in cell biology that has resulted from invention of the electron microscope and development of the techniques of immuno- and microchemistry, the electron microscope making visible cell structures a thousand times more minute than those visible without it, and microchemical methods making possible more and more progress in the chemical analysis of these minute structures.

Instrumentation (including science technology in general) has been given first place in this survey of important and promising avenues of research effort because it stands at the doorway to progress in science.

Further advances in the sensitivity of instrument types now in use, development of new types of instruments, and further adaptation for biological use of instruments used in other areas may be expected in the next decade, and, with each development, an explosion of new research in the corresponding scientific area. Great advances are to be expected in the coming decade in the adaptation of computers to medical and related biological research. The use of electronic computers for analyzing the complexities of interrelated biological data is in its infancy. Efforts will surely be intensified to develop improved methods for storing and retrieving scientific data, and for analyzing and interpreting them. The further use of computers in the analysis of data from x-ray crystallography of proteins and nucleic acids may be cited as an example.

Quantification and evaluation of the information-input capacity of the various senses may be achieved. Progress may be expected in the development of computer analogs or models for the simpler brain functions. More instrumentation will undoubtedly be developed for continuous measurement and recording of some of the many variables undergoing simultaneous change in the body in response to stress or other change in conditions, or to disease or therapeutic measures, both for purposes of research and for diagnosis and observation in medical practice. The use of computers in the further development of mathematical biology and for further progress toward ultimate automatic translation of foreign scientific literature may be expected.

Prosthesis. Related to instrumentation and associated techniques is the area of prosthesis—a term referring here, in the broadest sense, to artificial substitutes for, or aids to, body parts and functions. Further investigation directed toward the following objectives may be expected: developing artificial heart valves; improving extracorporeal blood oxygenating and circulating units; perfecting techniques for maintaining some part of the body (for example, a cancerous extremity) under a separate circulation with a high concentration of some remedial agent; devising a means of aiding or replacing failing kidney function; improving dental filling and bonding materials; devising a substitute (possibly tactile) for lost vision or for a lost sense of equilibrium.

Tissue and organ transplants. Research may be expected to continue on the problem of the rejection of skin grafts and organ transplants (for example, kidney), which now occurs except in cases where the recipient and the donor of the transplant have near-to-identical genetic backgrounds. Blood and bone-marrow transfusions regardless of serological type may be an associated development, if and when the general problem of immunological rejection of foreign tissue is ever solved. The same process of rejection is, of course, altogether beneficent when the body combats the "foreign tissue" of an invading pathogenic organism.

Tissue culture of bone marrow for purposes of transfusion may be brought nearer in the next decade. "Tooth banks" and tooth transplants are a hoped-for possibility.

Associated with the objective of successfully making tissue and organ transplants is that of regenerating lost tissues. A breakthrough toward controlled and useful regeneration of lost tissues in mammalian forms is hardly to be expected in the next decade, but as a long-term goal it will surely be kept in view, as research is continued on suitable lower species.

Human ecology and environmental health. A vast terrain remains to be explored in the general research area of man and his environment, both animate and inanimate—the mutual balance of environmental factors, beneficent and harmful, as they affect health and disease, longevity, performance levels, and even evolution. Important in this field are also the health interactions between human populations and interactions of these with other populations of animal and plant life.

One of the most important problems in environmental health is protection against unwanted radiation effects. Research has been pressed in the past several years and will surely be intensified in the coming decade.

The problem of making desired food additives safe and of determining a safe tolerance level for adventitious additives (chiefly residues left in food from insecticidal crop sprays) has become increasingly more acute in recent years, as agricultural chemicals and various substances required in food processing and packaging have multiplied. A vast amount of research will be needed to make sure the public is protected. The continued search for better biological tests that are equivalent to lifetime exposure for man is a prime necessity in such efforts.

Closely related to the foregoing problems are the problems of pollution of air and water by substances harmful to health. "Smog" is only one among many such harmful agents. Of prime interest is the pollution of urban air from products of the motor age. Identification of such products and knowledge of their long-term biological effects, with development of suitable control measures, are objectives of pressing importance. The possibility that some of these products of incomplete combustion may be implicated in the steadily growing incidence of malignancy of the lower respiratory tract gives such research added importance. Progress can be hastened through accelerated research in instrumentation; the need for such acceleration indeed pervades all research areas.

The atom bomb is a potential environmental hazard that warrants more health-related research than appears to be in prospect.

Cancer. The search for the cause or causes of human cancer and for means of prevention and better means of therapy has been pressed in recent years to an extent that almost entitles this to be called a crash program. Crucial knowledge is slowly but surely being accumulated. Demonstration of the virus etiology of a variety of cancers (including leukemia) in certain laboratory animals has renewed our hope for an early breakthrough toward control of this dread disease and has already led to more intensified research in this direction. There is also growing, if not indeed conclusive, evidence that carcinogenic substances can reach the body through the inspired air—evidence that relates the cancer problem to the general problem of environmental health. Research in this direction is being pushed and will surely be increased in the coming decade.

Host-parasite relations. The area of research on host-parasite relations encompasses all the relationships between man (and other animal and plant hosts) and the beneficent, neutral, and harmful plant and animal parasites that infest and infect, including viruses, bacteria, protozoa, and other parasitic organisms. An extension of the parasite concept can, of course, bring the invasive cells of cancer into the same category. Research will continue on a broad front on the pathogenic parasitic organisms, on their nutritional requirements and metabolic processes, and on the evolution of pathogenic forms and the development of pathogenicity in forms that were previously inactive (in the carrier state) or harmless or even beneficent (for example, the colon baccillus). The interaction between host and parasite, in particular the effects of the parasite upon the host and the mechanism of these effects, will continue to engage the attention and effort of research workers, as will the continued development of control measures, including antibiotics and other chemotherapy. The development of parasite resistance to such therapy in the course of an infection—a heartbreaking event—and the perpetuation of such resistant strains thereafter to endanger the lives of others are twin problems that will call for intensified further research. The restraint upon one parasitic population that results from the presence of another also deserves more study.

This research area is obviously one of extraordinarily broad scope, including as it does all the infectious diseases. A vast amount of work has already been done, during nearly a century of research, but the area remains at or near the top of any list where priority is determined by pressing need or promising opportunity.

Tissue immune reactions. Closely related to the great research area just discussed is that of the reaction of the body to substances foreign to it—a reaction of either defense or neutralization or acceptance. The preponderance of research in the past has properly been directed toward strengthening the mechanisms for defense, for here lies the greatest need; but when the defense is against a skin graft or a transplanted kidney donated to an individual in dire need, the same beneficent mechanism can act blindly as a liability. The next decade should see an intensification of work in both directions—toward strengthening the body's defense mechanisms on the one hand and toward holding them in check on the other.

Inseparable from the objective of strengthening the body's natural defenses is that of adding new defense factors, chemically tailored to general or specific needs. Research toward this end holds continued promise of future rewards.

Antimetabolites, antibiotics, other chemotherapeutic agents. Although they have been referred to incidentally in the previous discussion, antimetabolites, antibiotics, and other chemotherapeutic agents deserve special mention. Antimetabolites are among the most promising of the agents being tested for anticancer action, and they also offer continued promise in the attack on invading disease organisms. The last several years have seen the testing of all manner of chemical compounds, many thousands of them, for possible anticancer activity. The next decade will see a continuation of such testing and of research toward the development of new antimetabolites and antibiotics and of other compounds for effective chemotherapy.

Heart, circulation, and blood. We may expect further advances yearly in heart surgery and prosthesis, in techniques of localized perfusion, in diagnosis and relief of vascular insufficiencies of various body areas, in the control of clotting, in understanding and controlling the processes in hematopoiesis, and in our knowledge and control of the basic causes of atherosclerosis.

Reproduction. The well-being of the new individual will continue to be the dominant practical objective in research on reproduction, where progress will be dependent upon a clearer understanding of the processes involved and the factors that influence them. Reduction in fetal wastage and deformity (biochemical as well as anatomical) will remain an important immediate objective. Overpopulation and associated hunger in some world areas will continue to stimulate interest in developing more effective measures of birth and population control.

The brain. The outlook for brain research in the next decade is one of continuing investigation with the oscillograph, the electroencephalograph, and other instruments; of localized short-term and long-term brain stimulation and the placing of minute brain lesions, precisely localized; of continued exploration of the biochemistry and the pharmacology of limited areas and of brain secretion of hormones; of deeper delving into the biophysics and biochemistry of excitation in studies on single neurons (and other types of cells); and of continued efforts to extend the limited analogies between brain activity and the function of computers (as a class) in information storage, organization, and retrieval. Both in the field of neurology and in that of mental health, advancement toward the control of disease will be promoted by such research.

Behavioral science, mental health. During the next 10 years we may expect to see more research in which attempts are made to relate mental phenomena to the underlying biochemistry, biophysics, and endocrinology of the brain; more study of the behavioral patterns in the lower animals; and further study of the factors affecting and determining the course of development of the child, enabling him to assume the responsibilities of the adult as a member of society.

Cross-cultural and other studies are needed to determine the influence on mental health of such factors as patterns of thought and behavior, systems of personal and social values, the structure of the family and other social groups, patterns of interaction in the family and community, levels of aspiration in relation to the attendant environmental and economic potentials, and hygienic practices of populations. Such research will promote control of mental disease and control of the development of such patterns of deviance from social norms as alcoholism, accident-proneness, and juvenile delinquency. Research in some of these areas is practically in its infancy.

Aging. Research directed toward discovery of the fundamental processes in aging will be pursued further by investigators experienced in the field of cell biology. It may be hoped that the biochemical, biophysical, and structural differences between the aged and the youthful cell, and the effects of these differences, will begin to be understood.

As for the diseases so prevalent among the aged—heart and vascular and collagen disease (including excessive fibrosis)—and the "natural process" of progressive shrinkage of the various functional cell populations of the body with advancing age, no dramatic "breakthrough" is in sight; but as the coming decade advances, the slow accumulation and piecing together of bits of knowledge gained through basic research will surely bring us nearer to an answer to the problems of aging.

Cell and molecular biology. Reference has been made to improvements in instrumentation that have permitted examination of finer structural detail and chemical analysis of more minute portions of material than was previously possible. The electron microscope and the developments in microchemistry and in x-ray crystallography, together with technical developments in other areas (for example, immunochemistry), have opened up for study the single cell and its constituent structures. Researches on cell morphology, physiology, biochemistry, biophysics, pharmacology, pathology, radiobiology, and genetics are in progress and will undoubtedly be greatly extended in the coming years.

More detailed study of disease processes may be expected, with further exploration of the precise architectural structure of molecules in disease states as contrasted with their structure in the state of health. Although the importance of the precise architecture of molecules in biological processes has long been appreciated, particularly in the fields of enzymology and immunology, molecular biology as a research field is still in its infancy. It will undoubtedly grow in stature in the next decade, as its newly conceived sibling, submolecular biology, just begins to stir.

Nucleic acids. If the research areas in the whole of medicine and related biology were represented as mountain peaks in a vast terrain yet to be fully explored, the Mount Everest in that little-explored country would surely be "Mount DNA." Deoxyribonucleic acid (DNA) and ribonucleic acid (RNA) are the two nucleic acids that are found, singly or together, in the cellular units of all living organisms so far examined, from viruses, bacteria, and other plant forms through all animals forms up to and including man. The most challenging research in the future will be that directed toward the biosynthesis and function of these nucleic acids; for out of such studies will come the revolutionary answers to long-standing questions regarding the phenomena of genetic reproduction (replication) of cell structure and function and the biosynthesis of the proteins, the most important structural components of living systems. Possibly, too, from such studies will come the answers to questions regarding the fundamental nature of cell differentiation, the development in each individual of such different cell populations as those of nerve and liver from an original single cell—the fertilized ovum. New techniques in the study of the nucleic acids have been delevoped in the past 10 years, and it is certain that research on these compounds will be vigorously carried on for many years to come. This research is so basic that dividends can flow from it in almost any direction. One could well be in the direction of cancer control.

Genetics. Closely related to, even inseparable from, research on DNA and RNA is research on the gene population (the "genotype") in the original single cell from which a cell progeny is derived, whether this be a clone community of bacteria in a flask or a cell aggregate making up one human being. What genes are present in the parent cell, what factors determined their presence together, and what bodily attributes (including hereditary disease or susceptibility to disease) each gene or gene group controls—these are some of the challenging questions in genetics. New techniques for visualizing the entire complement of chromosomes in a single cell and identifying each by its peculiar characteristics now make possible a surge of new work on hereditary disease and susceptibility to disease. Control of the complement of genes with which each individual starts his existence is a visionary objective, even though probably unattainable as a goal.

The genetics of new pathogenic strains of organisms; of the first malignant cell to appear in an individual who develops cancer; of the development of resistance to chemotherapy in a viral, bacterial, or cancer cell population—these fields, too, present challenging problems on which more research is urgently needed.

Much of the promise of achievement yet to come from medical and related biological research rests upon the further development of interdisciplinary team work, now well under way. A more extensive development of great research centers for categorial and general research in the coming decade is a strong possibility.

Basic and applied research. The amount of applied research carried on from year to year in the coming decade should be in homeostatic equilibrium with the amount of (pre-existing) basic research, for each is dependent upon the other.

Applied research has as its objective some achievement that can be put to "practical" use in some way other than as a step toward further research. Basic research contributes new variables to science, quantitates them, identifies (and quantitates) new causal relationships between variables, and points out new spatial and temporal groupings of variables and new sequences in their changes in value.

The motivation and justification and the basis of evaluation are the same for applied research in general and for any one project in applied research: They are, respectively, the practical objective and the extent to which it is attained. For basic research, the motivation is scientific curiosity—an almost monastic dedication to the pursuit of learning. The justification (in the eyes of the onlooker, including the one who supplies the funds) is that the stream of applied research dries up unless it is fed by basic research. The merit of any one achievement in basic research is measured by the extent to which it clarifies pre-existing knowledge, contributes toward establishing a new generalization, or simply leads to new research.

The interdependence of applied and basic research has been pointed out. No matter what the practical objective of any applied research is, "spade work" (equated here with basic research), unless it has already been done, is found to be necessary. An enormous amount of basic research may yet have to be carried out before death rates from heart disease and cancer can be substantially improved. Thus, the need for applied research stimulates support of basic research, and the findings of basic research ultimately open the doors to more applied research. There is no reason to believe that this symbiosis will be in any way disturbed in the coming decade.

Notes

1. The data are being collected by the U.S. National Health Survey, which was begun in 1957.
2. This figure is not shown in Table 6 but can be obtained by multiplying entries in column 2 by those in column 3.
3. This can also be said, of course, of work supported by other agencies that have a similarly effective review mechanism.
4. "Research supported by the NIH" includes both grant-supported and intramural research.
5. Grateful acknowledgment is made here for the contribution of these scientists.

THE SCIENTIST AS PUBLIC ADMINISTRATOR

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SACRIFICE of a research career for one in research administration is common among scientists in the United States today.[1] It is paradoxical that so many men desert a highly respected, even idealized, way of life, for which their professional training and experience have prepared them, to enter an occupation whose very professionalism is suspect. The research administrator stands in an area of ambiguity and transition in the meaning of work: though he enjoys considerable prestige in society and salary and power advantages over researchers within bureaucratic organizations, the actual conduct of research has been at the core of the image of the good scientist.[2] If "a man's work is as good a clue as any to the course of his life, and to his social being and identity," [3] basic elements in the scientific career and the bureaucratic system may be illumined by understanding the changing of roles by the scientist. Is the scientist who becomes an administrator misled by ambition and then corrupted by power, or is he enabled to realize a fuller potentiality and make a unique contribution? To answer this, two subsidiary questions must first be examined: Why do men leave research to become administrators? Does the researcher-turned-public administrator remain always the scientist, or are his attitudes toward politics, science, and administration those of the administrator?

Where other sources are not indicated, the analysis presented below relies heavily upon a series of interviews with Washington area federal research administrators. Two dozen or so each in the National Institutes of Health, the Agricultural Research Service, and the Navy, plus some research scientists in these agencies and some Budget Bureau and Civil Service Commission officials, were interviewed. The administrators, most of whom had been trained as scientists, were representative of a variety of ranks and functions, though purposely bunched toward the top. Interviewees, none of whom objected to the taking of full notes with the guarantee of anonymity, spoke in response to a statement of the purpose of the research and specific questions appropriate to the person, agency, and level. The writer did not feel that it was impossible to discriminate qualities of openness and perspicacity in those interviewed. Conversational interviewing was no doubt facilitated by the cathartic value to many persons in talking about themselves to a detached observer, the ability of men within a formal organization to learn a good deal about close colleagues, the appeal of an objective research project to persons brought up in a society in which belief in science is pervasive, the interviewer's identification with a university and a Washington research institution, and the blessings bestowed upon the work by top administrators in each agency. The writer was deeply moved by the willingness of these men, manifested by a significant proportion of those interviewed, to examine themselves with probing honesty.

NOTE: The Brookings Institution, through an appointment as Visiting Professor, and the University of Massachusetts Research Council, through a grant, facilitated this study. Each is innocent of the consequences of its generosity.

[1] Of over 200,000 scientists registered in 1960 with the National Register of Scientific and Technical Personnel, about one-fourth listed their work activity as "management or administration." *Scientific Manpower Bulletin,* No. 17, April 1962 (Washington, D.C.: National Science Foundation, NSF 62–11).

[2] Harvey L. Smith writes that every profession operates in terms of "a basic set of fictions about itself," which "tend to concentrate the rewards of prestige in some areas of professional activity and to ignore others." "Contingencies of Professional Differentiation," *American Journal of Sociology,* 63 (January 1958), 413.

[3] Everett Cherrington Hughes, *Men and Their Work* (Glencoe: Free Press, 1958), p. 7.

Career and the Range of Satisfactions

Men often find reward in more diverse occupational activities than they would admit or suppose. For example, professionally active men may enjoy the armed services during wartime, perhaps because of release from responsibilities, competition, or meaninglessness, or because of comradeship, command, or subordination. Like the civilian professional-turned-soldier, a scientist who becomes an administrator was a complete man, not simply a "scientist," which is an abstraction, focusing on certain training, attitudes, and activities of the individual. Even one who has found satisfaction in research may sacrifice it without pain and find pleasure in other work.

Entry into a profession generally requires a great investment of effort and a major commitment by the neophyte, but is based on something of a guess. A youngster in the United States, if he is not a victim of poverty or physical or psychological deformity or cultural prejudice, may have a wide choice of occupations. Typically he makes his decision on the basis of vague knowledge of his own talents and of the requirements, actual work, and rewards of the various occupations.[4] He chooses early, with little experience and little real advice, even from well-meaning but generally permissive and uninformed parents. For instance, one who enjoys mechanical activities and does well in school courses presumed to be related to engineering may fix on engineering as a career. An early decision in this direction may mean, one study suggests, that a young man goes through engineering school before recognizing that he "would prefer to work more directly with people than with materials." [5] The same process may occur with the boy who enjoys science and does well in science courses, but who later feels that it touches only some of his interests and capacities, and would welcome the chance to develop his "human" interests as well. Administration is an outlet for these nonresearch interests.

Many scientists-become-administrators eased into administration gradually. Superordination begins naturally in the scientist's relationship to the student or laboratory assistant. Scientists regularly direct personally the activities of one or more others, and are used to not doing with their own hands everything professional that they seek to accomplish. Also, because modern science involves the bureaucratic organization of many men, large sums, and expensive equipment, it requires a great deal of formal administration at varied levels. Committees on grants and fellowships abound, giving scientists a taste of organized decision-making: the medical scientist assumes administrative responsibilities in the hospital; the university scientist is assigned to faculty committees; other environments offer special administrative occasions.

While directing a few others in research, the scientist may still be giving most of his time to his own research. The chief who directs a small group, though bearing administrative responsibilities, is still concerned with the substance of research; the higher level chief is involved primarily or solely in administration. Frequently despite intentions to the contrary, his own program of research is dropped completely. He is concerned with promotions, hiring, space, equipment, assistants — the whole realm of "support" of research. These support decisions have great practical consequences for the subordinates and real psychological significance for them. Not to be supported adequately, where resources are apparently available, is an invitation to trim sails or leave. To make such decisions is to administer, to wield power.

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[4] Cf. Morris Rosenberg, *Occupations and Values* (Glencoe: Free Press, 1957). Richard L. Meier, "The Origins of the Scientific Species," *Bulletin of the Atomic Scientists,* 7 (May 1951), 169, 173, confirms the lack of information in the choice of profession by scientists, but discerns a change in this situation.

[5] Eli Ginzberg, *et al., Occupational Choice; An Approach to a General Theory* (New York: Columbia University Press, 1951), p. 191.

In sum: (1) scientists often find satisfaction in the administrative function — perhaps to their own surprise, after the dreams of youth, the indoctrination of graduate school, and the challenges of the laboratory; (2) scientists easily become increasingly involved in administrative activities in the course of a normal research scientist career in the United States today. The administrative option is generally close at hand, and no abnormality of motives or circumstances is required to reach out for it.

Motives for Change

Chance plays a part, and no doubt some scientists who would have done well at administration are never offered the opportunity, while other researchers enter administration really by accident. However, something more than chance is at work. Discussions by scientist-administrators of their own motives and careers and of those of other scientists-turned-administrators whom they have known well suggest the following classification of motives and circumstances underlying the move from research to administration.

Men of quite varied ability enter administration because they seek advancement. Sometimes a strongly driven scientist seeks the salary, prestige, and power that go with hierarchical rank. Such a man may or may not deeply enjoy science, but he wants the obvious marks of success, whatever he does. On a more modest level is the man of decent but limited talents, who wants promotion and finds it available only by entering administration. Probably he dangles in mid-bureaucracy, making minor contributions, as he would have done had he remained a researcher. At all levels of competence, then, are men who give up research to gain the rewards, modest or substantial, which administration offers them. We can, however, specify circumstances and motives beyond simply the desire for advancement.

First, there are men who have not achieved real scientific distinction, never are likely to, and know it well. They may be perfectly adequate researchers, but they come to see that they can never make a really major contribution: "I will never win a Nobel prize." They do not hate administrative routine, nor do they shy away from authority. Administration seems to offer release from the strains of not succeeding in science on the grand scale of graduate school dreams, and it brings money, prestige, and other rewards, so they take the opportunity when it comes. Though they probably enjoyed many aspects of the scientist's life, they are not undergoing a traumatic experience when they leave research; often they see this in themselves. Colleagues, too, recognize this, and may resent it if these former researchers become aggressive administrators, confidently stating what research should be undertaken, how it should be organized, and who should be rewarded.

Second, there are men who were good, even very good researchers, but who, through advancing years (which may mean no more than forty years of age), begin to lose their imagination, vigor, confidence.[6] Younger men coming into his field with new and more sophisticated techniques (for example, mathematical learning of a sort he lacks) may make a man feel outmoded or outclassed. Similarly, if his specialty loses prestige and glamour while new branches attract energy and attention, he may feel he is an unexciting, old-fashioned fellow. Scientific research emphasizes drive, technique, imagination, ability to uncover new truth. In these qualities, young men often match or excel their elders. The advancing man — who is still only middle aged — may turn for haven to administration, which is assumed to require maturity, experience, and judgment, rather than continuing research prowess.

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[6] Some scientists continue productive research late, some may start late, some have a second flowering, but scientist-administrators testify strongly that tapering off is not simply a myth, however complicated the basic physiological, psychological, and social causes may be. Anne Roe, *The Making of a Scientist* (New York: Dodd Mead, 1953), pp. 45f., suggests the possible importance of early maturation and rapid decline of specific abilities as a factor in movement from research to administration. Cf. C. P. Snow, *A Postscript to Science and Government* (Cambridge: Harvard University Press, 1962), p. 16.

People may say that "we have lost a fine scientist," but perhaps he has already done the best work of which he is capable. With his knowledge of science, his respect for good research, and his own high standing, he may usefully head a research program. His appointment may be good for himself, for the morale of his subordinates, and for the agency and program.

Third, there are men who are so good at administration that strong pressures are recurrently brought on them to assume administrative responsibilities. Such a man cannot easily remain within an organization and resist these pressures; perhaps against his will, he is forced from the laboratory more and more into administrative duties. He probably is a good verbalizer and a wide reader, thus impressing his more limited colleagues. He probably has great energy and self-confidence, a touch of charisma. He may be an outstanding researcher as well, or at least competent enough to be well respected. He may genuinely miss the laboratory and occasionally give up responsibility to return to research, but usually not for long. Actually he probably enjoys both administration and research and will never fully resolve his dilemma. Granted great prestige for both his research contributions and his high administrative post, he may nevertheless feel dissatisfied, because he is not keeping up with his area of science adequately and because his own research work has to be sacrificed. Being "strong" men, these administrators are likely to arouse intense loyalties or antipathies; they offer respectability, direction, and drive to an organization.

Fourth, there are men who believe strongly in a program, method (for example, interdisciplinary research), specialty, or particular theory. For such men, administration affords the means toward specific scientific or social goals. "Helping others to do their research" is a common rationalization among research administrators of their decision to enter administration, but before one can take this seriously as a motive, one must see it particularized. It is real, probably, when it is "helping others" to do something specific that the administrator thinks they ought to do, perhaps despite their own wishes, because it is important.[7] These administrators may use their organizational ability to accomplish large tasks, driven by faith in the outcome.

Scientists who come from outside government directly into public research administration may sacrifice not only research but the academic life. What has been said above is relevant, but we may also indicate as possible factors: interest in being at the center of a program, with the opportunity to act authoritatively (as in passing on grants) within one's scientific specialty; higher than academic salary; the special environment of Washington; and a change from normal routines of life. It is sometimes an honor to be asked to come to Washington to assume responsibility for a program in one's scientific area, and often these men must have had some taste of administration to whet the appetite.

These categories require a sympathetic reader, who knows that career is "a sort of running adjustment between a man and the various facts of life and of his professional world," "his laying of his bets on his one and only life," "a set of projections of himself into the future." [8] We recognize overlapping and irregularity, and will put no man on the rack to make him fit the categories.[9] It is best to emphasize the real variety of men and motives. Administrative power is for some a quiet haven and merely a job, for some an end in itself and a joy, for some a means to the top, and for some a sober duty — a self-sacrifice — to science, to colleagues, to noble purpose.

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[7] One able administrator entered government service at real sacrifice, including his own research, when offered a fine new facility and a large sum to recruit professional personnel; he was told he might do what he wanted with this new program. To turn down the opportunity, he explained, would be "like refusing a chance to go with Christopher Columbus because business is pretty good now."

[8] Hughes, *op. cit.*, p. 129.

[9] Dwaine Marvick, *Career Perspectives in a Bureaucratic Setting*, Michigan Governmental Studies, No. 27 (Ann Arbor: University of Michigan Press, 1954), categorizes high-level bureaucrats in an unidentified agency (which sounds rather like ONR) into institutionalists, specialists, and hybrids, distinguishing those who are "place-bound" from those who are "skill-bound" from those who are "free agents." Cf. William Kornhauser, *Scientists in Industry; Conflict and Accommodation* (Berkeley and Los Angeles: University of California Press, 1962), pp. 118–30. Focusing on autonomy versus integration of professional activity within organization, this study is useful respecting the strains between professional

THE ADMINISTRATORS' THINKING

What attitudes toward politics, science, and administration characterize the scientist-turned-public administrator? Were public research administration a well-established profession (or a group within a profession of research administration or public administration), shared attitudes might stem from professional school training, where traditions are inculcated, and perhaps from a professional association. Public research administration has not yet developed, as have the traditional professions, to the point where it provides a practitioner with "a subculture and an identity." [10] Each agency influences research administrators toward distinctive attitudes. Agency "personality" affects recruitment, morale, attitudes toward work, standing in the scientific community, and so forth. The attitudes of the administrator may reflect such characteristics of his agency as: (1) responsibility for an intramural or an extramural (grants or contracts) research program or both; (2) a programming approach to an extramural program or a more passive approach, responding to unsolicited grant applications; (3) treatment of the research program as an end in itself or as a means to a non-research goal; (4) emphasis on scientific competence or on administrative or political skills in advancing people; (5) emphasis on direction of or autonomy for researchers; and (6) intensity, the pace at which men drive themselves and others. The agency atmosphere, as well as the individual's experience and personality, contributes to shaping his job and his attitudes.

In light of the tenuous professional status of research administration, the distinctiveness of agency character, and the limited interaction and mutual influence of public research administrators, similarity of views may be traceable to independent attainment of truth or to the influence of similar experiences individually encountered.[11] The most obvious similar experiences are: contemporary American culture, probably middle class; college education, including graduate school; science training and research experience; federal government service; and research administration duties. These similarities of background and function in reasonable men often lead to similar views. Though the range of attitudes is wide, we can suggest the dominant view, the clustering toward one or another pole on a spectrum.

The Creed of the Research Administrator

On the basis of published and private statements we can draw up a "creed of the scientist" and a "creed of the public administrator," in terms of which to align the

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and bureaucratic goals. Cf. Simon Marcson, *The Scientist in American Industry; Some Organizational Determinants in Manpower Utilization* (New York: Harper, 1960), pp. 65–70, respecting the reasons why industrial researchers move into administration.

[10] Hughes, *op. cit.*, p. 129. One can distinguish the institutional arrangements (training schools, examinations, licenses, professional organizations, codes of ethics) from the psychological aspects (a feeling of lifetime commitment to the work as an art embodying a respected tradition of service) of a profession. Serious students of professions are hesitant to insist upon any precise definition of a profession, through various typical qualities or evolutions can be discovered. Cf. A. M. Carr-Saunders and P. A. Wilson, *The Professions* (Oxford: Clarendon Press, 1933), pp. 284ff.

[11] Despite real reservations one must have about his and other sociologies of knowledge, there are valuable insights into the relation between experience and ideas in Karl Mannheim's *Ideology and Utopia; An Introduction to the Sociology of Knowledge*, trans. Louis Wirth and Edward Shils (New York: Harcourt, Brace, n.d.), chap. I especially.

research administrators.[12] Each "creed" represents a distillation of widespread attitudes, not the views of any particular individual. To meet their polarizing function in this analysis, the assumptions of each creed are stated more explicitly and coherently and with less qualification and ambiguity than most scientists or administrators would state them. If the "creed of the scientist" inaccurately states current attitudes, it loses value in describing attitude changes in those who leave research for administrative duties, but retains analytic value as a pole in static description of the attitudes of these administrators.

The "creed of the scientist" includes these assumptions: (1) scientific knowledge is the most certain knowledge; (2) science is the most profound kind of knowledge, revealing the beauty and meaning of the universe; (3) science is the most useful kind of knowledge, leading to invention and technological advance; (4) common-sense knowledge is unreliable; (5) scientists are objective, seeking only truth; (6) scientists are essentially men of peace, and science is a truly international language; (7) the most intelligent men today are scientists, and through the scientific method they can contribute to the solution of world problems; (8) we are in the "age of science," and science is basic to a good education; (9) more money and manpower and public appreciation should go to science; (10) politicians and administrators, lacking scientific training and unused to rational methods, are unreliable; and (11) the less political or administrative interference there is, the better the research program.

The "creed" of the public administrator includes the following: (1) common sense knowledge and "feel" keep agencies and programs going; (2) experience and sensitivity are the key qualities required for successful human dealings; (3) absolute truth would often be a foolish policy in human dealings; (4) the world is inevitably highly imperfect, as is human nature, but a good program is a useful social contribution; (5) it is difficult to change people's beliefs or patterns of behavior, and a proposal for major change requires strong justification; (6) any action which will anger a politician or group is suspect; (7) politicians, though they are sometimes apparently irresponsible, are not bad people, and one can generally reason with them; (8) experts and specialists vastly overrate their own importance, and are difficult to work with; (9) one can adjust to and live with most of the burdens an agency suffers, though outsiders do not realize the difficulties encountered; and (10) superiors often fail to give the program adequate support; subordinates, on the contrary, often fail to take an over-all view of the agency's needs, being enthusiasts for their own program. The creed of the scientist may be classified as essentially politically utopian, that of the administrator as politically realistic.

The federal research administrators interviewed seemed, above all, realistic in their attitudes toward politics, science, and administration. Most had no expectation of radical improvement in human nature or human relations through the techniques of science. There was little talk of science as a means to solving world problems or of scientists as new public leaders toward a better life. There was, of course, interest in and respect for science and confidence that scientists can often solve the sorts of problems for which their training equips them. There was natural agency enthusiasm and pride, but of the sort that any administrator is likely to express in talking about his program to a stranger.

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[12] Cf. Walter R. Schilling, "Scientists, Foreign Policy, and Politics," *American Political Science Review,* 61 (June 1962), 291–96, respecting the "policy perspectives . . . which seem moderately characteristic of many scientists, most of them physicists, who have participated in national security policy in recent times." Meier, *op. cit.*, compares chemists (politically moderate), engineers (conservative or apolitical), physicists (politically radical), and biologists (somewhat more varied). Respecting sixty-four top American scientists active in research, with comparisons among biological, physical, and social scientists, cf. Roe, *op. cit.* The journals *Science* and *Bulletin of the Atomic Scientists* are useful sources of the views of "spokesmen" for science. C. P. Snow's novels, *The Search* (London: Macmillan, 1958) and *The New Men* (London: Macmillan, 1954), are interesting on many of the questions discussed in this paper. Respecting American public administrators, Paul Appleby's works are especially useful. Kornhauser, *op. cit.,* pp. 58f., 138f., found that research managers in industry, especially at higher levels, tend to be oriented to bureaucratic, not professional, norms.

Politics

Research administrators interviewed had a realistic view of politics, one close to the creed of the administrator. Especially as one rises in the hierarchy, administrators are not generally marked by despair over political interference. There is a phrase popular among Washington officials: "We can live with it." This is the typical attitude toward politics. Politicians are seen as men who are, on the whole, decent, intelligent, and cooperative, but whose political commitments and goals affect their behavior. The influence of politicians does not depress, disgust, or excite the research administrators.

Attitudes toward politics vary to some extent with the political experience of the particular agency. The National Institutes of Health have done remarkably well with Congress. Heading their appropriations subcommittees in House and Senate have been sympathetic, able supporters. Congress has consistently increased a budget which the executive has held stable. NIH administrators can scarcely help but think that Congress is kind to science. Department of Agriculture research programs have benefited less and suffered more from congressional intervention, so there is a less universal and unqualified confidence in Congress. There are differences, then, between those who have been very lucky and those who have had mixed luck.

Top administrators are not likely to be completely uninvolved in politics nor to have a complete distaste for it.[13] Many are quite skillful at it and genuinely enjoy it. Others emphasize their research interests and the scientific aspects of the job, as a protection against too great political involvement; but only in part can an administrator shape his role in this matter. Whether they glory in it or simply make the best of it, top administrators are close enough to politics to understand something of it. At lower levels, the administrator may have little or nothing to do with what he can identify as politics; it is a world that he knows by hearsay, and often one he would just as soon avoid. Some research administrators at lower hierarchical levels, not long removed from research positions, expressed an uncritical confidence in science and a complete distaste for politics and for mere common sense. One can hazard the guess that their attitudes will change as they gain administrative experience and climb the hierarchy, though a few men at high levels joined utopian rhetoric with administrative talent.

Scientific Freedom

Freedom usually bulks larger in language than in fact, because it runs counter to other values when decisions must actually be made.[14] Some scientist-administrators at high levels manifest a reverence for research and for the individual. Ill disposed to interfere, they have a deep sense of using and nurturing the talent and interests natural to a man, rather than regarding him as clay to be shaped. Where, for example, it is easy to punish a man who has gone stale, they seek to refresh him; he is too valuable to be regarded as finished. Many administrators see their job as one of protecting research subordinates against "improper" — the understanding of which varies among agencies — political or administrative demands from outside or above.[15] They serve as a buffer, warding off extraordinary political pressures and ignoring or modifying administrative regulations that would impede good work.

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[13] It is not unknown for federal research administrators deliberately to assume a scientific posture in congressional appearances, de-emphasizing administrative and political responsibilities and sophistication.

[14] This produces oddities, such as a naval laboratory where many members talk of the university atmosphere they enjoy; yet security regulations are very tight, there is much applied research, and the Navy is clearly present. Reference to a university no doubt helps morale, and for at least some men there is very wide scientific freedom.

The research administrators' attitudes toward scientific freedom were generally matter-of-fact. In units with a specific task, it is presumed that scientist members should contribute toward that purpose. The right to do "whatever you want" is defended only if the agency is not one that has to produce short-run practical results. In almost all instances, what the researcher undertakes must fit within the general scope of the agency program, though that may be very broad. In research units such as those within some of the operations agencies of the Navy, the justification of research by its practical results is, understandably, deeply embedded. In these cases, research blends into development and engineering and production, and it always faces toward the needs of the Navy. Individual curiosity is less likely to be revered in such a setting than in an agency with longer-range responsibilities and more academic connections, such as the Office of Naval Research, which contracts extensively with university professors. Even in the practically oriented Navy units, however, some research is sponsored which has a quite delayed military utility; administrators see the need for scientific discretion in such cases, but the basic assumption is that science can serve military needs.[16] Similarly, the organization of Department of Agriculture research largely by crop and animal specialties suggests that administrators generally expect practical findings within specified areas of work, though there have been serious attempts to foster basic research. Within units involved in more applied research, administrators usually feel less vividly the researcher's right to control his own work, and may recognize political or administrative influence on the researcher as more legitimate.

Research administrators' attitudes toward scientific freedom varied significantly but were, in sum, practical, decent, seldom romantic, sometimes hard. Certainly they assume the legitimacy of organization authority and the value of the goals pursued by the organization. They assume that some planning is possible in science and that their agencies do have specified areas of responsibility. They believe that not every scientist is highly original or likely to make a fine contribution if left alone; that the best men may easily be left to do largely what they want; that others will not suffer from some direction; that some must be led. The attitude is, in general, fairly realistic.[17] As one descends to the level of the administrator who is still a practicing researcher, general principles and policies respecting scientific freedom seem to be less important than the personality of the administrator, his attitudes toward and working methods with his subordinates. A first-rate scientist, who insists on freedom, may be a tyrant to his subordinates.

Research Administration

Attitudes toward research administration vary markedly with rank. Those first entering administration still identify themselves strongly as scientists, more specifically as organic chemist or the like. This identification with an area of science clings tenaciously. It holds together the two halves of a man's professional life. Many a research administrator will, after hesitating over the question, decide he is a "biology administrator," or something of the sort, when asked his occupation. Quite near the top, membership in professional associations and some attempt to keep up in the field are often still found. There is a clear lessening of the real involvement with a scientific specialty and a clear increase of interest in and feeling for administration as one ascends the hierarchy. It seems certain that as the younger men rise their attitudes change.

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[15] Concern by research administrators over public pressure for quick results in medical research is apparent in Senate Committee on Appropriations, 86/2, Hearings, *Departments of Labor and Health, Education, and Welfare Appropriations for 1961* (Washington, D.C.: G.P.O., 1960), I, 751, 843ff. Congressional pressure on research administrators, in terms of geographic areas and commodities to be emphasized, is amply evident throughout Department of Agriculture appropriations hearings.

[16] This is analogous to the approach in industry, where one also finds a wide range of attitudes toward discretion for researchers, but faith that research does pay, either in the short or long run.

[17] If one denies the authority of the organization and the value of its goals, one could defend much broader freedom. For analysis of the proper limits to scientific freedom and of present federal practice, cf. my "Scientific Freedom in Government-Sponsored Research," *Journal of Politics,* 23 (May 1961), 212–30.

Tested by regular interaction with significant mutual influence, or by their own conceptions, the federal research administrators are not a group.[18] There are some real groupings within the whole, especially among those, of whatever agency, concerned with a particular scientific area or problem. There is a sense of shared occupation, common problems, and need for cooperation among some of those in charge of grants programs. There are a number of formal organs that bring administrators together, but generally these relations do not go very deep.

The most important relations of these administrators are usually not with research administrators in other federal agencies. In the National Institutes of Health, for instance, relations with the medical schools are of primary importance, far more important than relations with most federal research administrators. In the Agricultural Research Service, relations with the state colleges of agriculture are of great significance, but most agricultural research administrators have few contacts with military research administrators. Each agency has its clientele and colleagues outside government. The National Science Foundation, the most obvious central unit, is not a major influence in government research agencies. Neither in their own conception of themselves nor in terms of talking to and influencing each other in daily operations are the federal research administrators really a group.

As their movement into administration is made easier, so their identification of themselves as administrators is made more difficult, by the weakness of professional bonds among American public administrators.[19] Federal research administrators generally hesitate to call themselves "administrator," do not become involved in public administration as a discipline or a conscious profession, and do not have great confidence in formal management training or theory. Administration often seems a kind of accident which has altered the science career upon which they set out. Some do turn to the literature of administration for information, once they have taken an administrative post, and an increasing number of younger men undergo formal research management training, but they tend to trust common sense and their "feel" for research administration. Their natural abilities, particularly their knowledge of people and their research background, seem to them the most important qualities. They administer "by doing." Sometimes, especially at the top, they see the stark truth, that they have changed from scientist to administrator.

THE SYSTEM AND THE CHALLENGE

Is the movement of scientists within the federal service from research into research administration desirable? Are we corrupting good scientists and losing the potential fruits of their work, or are we simply recruiting administrators from the best source? Knowledge of their motives in changing and of their attitudes toward science and public administration provides groundwork for an answer.

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[18] Evidence of a growing sense of profession among industrial research administrators, who are also former researchers, is cited by George A. W. Boehm, "Research Management: The New Executive Job," *Fortune,* 56 (October 1957), 165. In training for some professions, such as librarian, engineer, and educator, apparently there is considerable concern with the administrative future of the successful professionals. Cf. Hughes, *op. cit.*, p. 137.

[19] The strongest loyalties in the U.S. civil service have been to specific agencies and clienteles and to traditional professions or technical skill groups, rather than to public administration as a profession. Cf. The American Assembly, *The Federal Government Service: Its Character, Prestige, and Problems* (New York: Graduate School of Business, Columbia University, 1954); Paul T. David and Ross Pollock, *Executives for Government; Central Issues of Federal Personnel Administration* (Washington, D.C.: Brookings, 1957).

The Research Career Opportunity

There should be reasonable opportunity for promotion for a scientist who chooses to remain within research, rather than to enter administration. This has not always been the case.[20] Some men with no special taste or talent for administration have entered it as the only way to advance to higher rank and salary. Sometimes quite good men have become involved in administration because it was their desire or the desire of their superiors that they be further honored, and only administrative posts were available to honor them. The strictly research career should always be attractively open for the man of high research talent, and even for one of moderate talent who has little administrative potentiality.

There has been encouraging movement in this direction recently in the federal civil service system. In June of 1960 the U.S. Civil Service Commission issued a "Guide for Evaluation of Positions in Basic and Applied Research." Its origins lie partly in a growing belief that the emphasis on position, rather than incumbent, has been too rigid in federal personnel administration. Two attorneys or two researchers may be doing strikingly different quality work, despite identical job descriptions. Attempts in the Department of Agriculture to promote researchers on the basis of personal attainment, without respect to supervisory responsibilities or similar factors (that is, without really changing the work they are doing) were especially influential in formulating the new program.

The Civil Service Commission guide applies only to those conducting "professionally responsible research," which includes basic and applied research, not development or testing. The guide is not intended for those who are "monitoring research contracts" or for positions emphasizing administrative responsibility; it suggests eight subordinates as a rule-of-thumb upper limit on posts emphasizing research capability rather than administrative skills. The guide "is based on the thesis that while supervision is *one ladder* to high level responsibility in scientific work, *there is another ladder* — the ladder of personal creativity and scientific contribution."

Typically in American personnel administration the job or position — the set of duties which are assigned to the occupant of the post — is central; interchangeability among incumbents is assumed; rank and pay attach to the job, regardless of who fills it or how well.[21] The new guide breaks with this idea quite explicitly, for "where the nature of the research situation involves a high potential for original and creative work, the position may be performed at any one of several levels, depending upon the level at which the incumbent is capable of working."

Four factors are to be considered in determining a researcher's grade: (1) the research situation or assignment, which focuses on the "inherent difficulty and complexity of the research problem"; (2) the supervision received, which involves the discretion enjoyed by the researcher in selecting problems and conducting investigations; (3) guidelines and originality, which pertain to the novelty of the problem, the precedents for it, and the contribution to theory or methodology; and (4) the qualifications and scientific contributions of the incumbent. An individual is awarded up to ten points for each factor, but double value for the fourth; totaling the point score, one can convert it into a grade level for the researcher. The commission has urged that rating of researchers be done within the various research agencies by panels with joint researcher-position classifier membership. Early experience in a variety of agencies has been that the scattering of ratings within a panel is not so serious as to make the system unworkable,[22] though of course a numerical rating scheme has an artificial air of precision about it.

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[20] That scientists "all too frequently" have to take administrative assignments in order to gain promotion is attested by the former Special Assistant to the President for Science and Technology, James R. Killian, Jr., "Improving the Government's Scientific Service," U.S. Civil Service Commission, *Recruiting Scientists and Engineers for the United States Civil Service,* Report of Proceedings, Conference on Scientific Manpower, April 28–29, 1959 (Washington, D.C.: G.P.O., 1960), p. 20. Industrial research laboratories seem to have moved toward the "two ladders of promotion" principle. Cf. Boehm, *op. cit.,* p. 222; Kornhauser, *op. cit.,* pp. 135–49; Marcson, *op. cit.,* pp. 29–34. Cf. Victor A. Thompson, *Modern Organization* (New York: Knopf, 1961). The relation between specialists and general managers is central in Thompson's book. He argues for "two equal salary scales, one for specialists and one for the hierarchy."

[21] Harold H. Leich argues that "the differences are becoming smaller in the United States and that, in many essentials, placement systems centering on rank-in-the-man and those centering on rank-in-the-job are now similar." "Rank in Man or Job? Both!" *Public Administration Review,* 20 (Spring 1960), 92. Cf. Truman G. Benedict's comment (*ibid.,* 21 (Winter 1961), 55–57), urging that rank-in-the-person should dominate, thus focusing on individual capability.

The guide and the thinking behind it offer promise of promotion to those who remain in research posts. Only actual practice will permit us to say for each agency whether the research road or the supervisory road leads to rank and pay. Experience will show also whether the two-track system encourages the advance of second-rate men in administrative posts, where first-rate scientists may formerly have trod. It may save some first-rate researchers for research. It may enhance the morale of researchers who no longer feel that administration is the only way to agency recognition. It may make easier for scientists belief in the justness of the system within which they work.

The Contribution of the Administrative Choice

What are the consequences, for the individual who chooses and for the system within which he serves, of the movement from research to administration? We grant the possibility of attacking the most irritating aspect of the system, by making promotion freely available to those who remain researchers. We assume that the great majority of research administrators are not within the outstanding to near-genius class. Many of those who move into administration would not have made major and irreplaceable contributions to science through continued research.

It is impossible to judge confidently the actual value to the organization of the scientific background of these administrators, because the individual is not separable into parts. Non-scientist administrators of business-type functions (personnel officer, contract officer, budget officer, executive assistant) work within research agencies effectively, on the general assumption that they assist but do not command scientists and that they deal with business questions, not scientific questions. These distinctions are not without meaning, but they are deceptively simplified. There are even instances of non-scientists dealing with research grants, apparently effectively. A bright, immersed layman can acquire over time a startling familiarity with the language, people, and problems in an area of science, without any corresponding ability to contribute to the body of scientific knowledge. Research administrators perform functions similar to administrators in other agencies; they tend to exaggerate the differences in problem areas such as budgeting and civil service regulations. The real difference is not only that scientists do non-routine creative work, but that they believe in their work and in the reality of a community of professional colleagues.

The scientific background of research administrators may be valuable in providing technical knowledge, foresight, communication ability, and status. At a fairly low hierarchical level, the technical knowledge and skills of the supervisor are important. This is the realm of the part-time administrator who also does research. At his best, he has much of the teacher about him. As one ascends, this kind of contribution decreases. At middle levels, former researchers who are now administrators sometimes have difficulty recognizing how small their substantive scientific contribution is.

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[22] Cf. H. Alan McKean *et al.*, "A Rating Scale Method for Evaluating Research Positions," *Personnel Administration*, 23 (July-August 1960), 29–36. Respecting the program in the Department of Agriculture, cf. House Committee on Appropriations, 86/2, Hearings, *Department of Agriculture Appropriations for 1961* (Washington, D.C.: G.P.O., 1960), I, 159; House Committee on Post Office and Civil Service, 86/1, Hearings, *Supergrade and Research-Scientific Positions in Various Federal Agencies*, August 20–21, 1959 (Washington, D.C.: G.P.O., 1959), pp. 3ff. Respecting the Scientist Administrator position, cf. Board of U.S. Civil Service Examiners, National Institutes of Health, "Professional Careers for Scientist Administrators in the Administration of Research and Training Grants, Awards, and Contracts in the Health Sciences," Announcement No. 227B, April 26, 1960 (Washington, D.C.: G.P.O., 1960).

Technical competence fades into a vaguer skill in program-planning. One hears the claim that the scientist-turned-administrator can discern the opening areas, the frontiers of science. No doubt the ability to do this varies considerably, but it is difficult to accept the view that, compared with researchers, most research administrators possess special foresight into the directions that science should take. They have the advantage of an organization position which affords a useful perspective, facilitating breadth of view. However, the practicing researcher has the benefit of being personally involved in ongoing research; his own wits are sharply challenged. Presumably the former researcher might at least choose among competing views more confidently than the layman. If the administrator claims, not that he sees the future of science clearly, but that in applied research programs he is familiar with the practical needs which the program ought to be serving, the advantage of a research background is not incontestable.

A former researcher, because of his experience at science, probably understands and communicates subtleties of the scientific life. At all levels, "personnel" decisions — when to hire, fire, promote, support, discipline — and the whole realm of how to influence a subordinate to do something, are of major importance. Here one's background in science is useful in understanding the *modus operandi* of scientists in a research organization, especially what uses of authority are legitimate. Common professional background is often confused, however, with experience within a particular agency and understanding of its procedures and customs, which a layman can acquire. A scrupulous layman may learn what a superior can appropriately tell a subordinate in a research agency without destroying the loyalty, dignity, and usefulness of the subordinate. Despite their background, not all former researchers recognize the proper limits to authority.

Researchers demand scientists as superiors. In the line of command at all levels in research agencies, the symbolic value of scientists is great. The respect in which they are held as fellow scientists, rather than mere bureaucrats, transforms authority into something more than brute strength. It is not unknown for a research administrator to say that his management aides may pass favorable decisions to researchers, but that only he will tell his subordinates bad news.[23] However dubious the more strident claims of value for technical knowledge, program-planning ability, and sensitivity to the way scientists think, the elements of truth in them and the symbolic value of scientists as administrators are important. There are ample opportunities for using non-scientists in research administration, especially in staff and auxiliary positions, but we will continue to require former and part-time researchers who will take administrative posts.[24]

If the research ladder to promotion remains open, there seems no reason to deplore the movement of some scientists in truly voluntary manner into administration. (We assume that the alternative is not the abolition of administrative posts, but the use of non-scientists in them.) It serves a useful function for scientists who, despite their training and experience, are not finding much satisfaction in research. It serves a need in scientists with great drives to hold power. It permits scientists with a vision involving large resources to attempt to organize toward its achievement. It supplies research administrators who have a background and reputation in research and in government and in the particular agency or area of work. The movement of some researchers into administrative posts is not a bad thing for the individuals involved or for society.[25]

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[23] However, cf. Norman Kaplan, "The Role of the Research Administrator," *Administrative Science Quarterly*, 4 (June 1959), 28, 33. By "research administrator" he means a management official, not a scientist-administrator. He reports instances of making this business administrator the scapegoat for unpopular decisions which the scientific director actually has made.

[24] Cf. House Committee on Appropriations, 86/2, Hearings, *Departments of Labor and Health, Education, and Welfare Appropriations for 1961, Department of Health, Education, and Welfare (Public Health Service)* (Washington, D.C.: G.P.O., 1960), p. 20, Surgeon General Burney's view that a mental hospital should be headed by a psychiatrist, rather than a lay administrator, because the whole environment of the hospital is important to rehabilitation. James L. McCamy deplores the myth that scientists and science are different from other men and functions, and criticizes the separation of science from policy-

Corruption

The greatest danger in converting researchers into administrators is the danger to the individual scientist. Non-scientist academicians often assume that scientists have fallen into arrogance, because of their superb techniques, their elegant and comprehensive theory, their great accomplishments in league with technology. Public statements by scientists who speak upon political and international affairs are frequently not reassuring of the scientist's sense of his limits. However, the scientist-administrators to whom this writer spoke were seldom marked by arrogance toward politics. (It is true that the group was composed rather heavily of biologists, who are reputed to be more moderate than physicists.) They were generally humble about their range of knowledge and their possible contribution to national and world affairs. Mostly they were political and administrative realists; theirs was the creed of the administrator, not of the scientist.

The scientist manages well the transition from researcher to administrator. He is not too utopian or too involved in scientific research to learn the art of public administration. As public administrator he develops skills in coping with political and administrative problems, in handling power. He deals with administrative superiors and subordinates, budget officers, administrators in allied and competing agencies within and outside government, interest-group leaders, and sometimes congressmen and political officials. Fingering its tools, he learns to value power as the means to varied ends.

Serving as administrator enhances the political and administrative understanding of the scientist, but he loses something. The scientist-turned-administrator cuts his ties to the disciplining experience, the personal responsibility, of specialization, research, and professional discourse. While becoming a political and administrative sophisticate, he may become less wise in matters close to the heart of science. He may give in to an expansive feeling of guiding the growth of science, playing the architect,

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making; they should be joined in government for all functions at all levels. *Science and Public Administration* (University, Ala.: University of Alabama Press, 1960), especially pp. 82ff. He insists, accordingly, though briefly, that administrators for science should be chosen without respect to whether they are scientists, simply on their ability as administrators (pp. 115–16). Scientists often are without skill in sensing social values, so lacking in administrative talent, he suggests (pp. 172f.).

[25] Cf. my discussion, "A Public Place for American Science," *Virginia Quarterly Review,* 37 (Summer 1961), 398–413. However, cf. William D. Carey, "Research, Public Policy, and Public Administration," *Public Administration Review,* 9 (Winter 1949), 53–63. He warns (p. 60) that "we are following the shortsighted policy of bleeding the laboratories of their skills," and suggests (p. 62) that we should "free the scientist for the work which he is most capable of doing by staffing administrative posts with nonscientist personnel oriented in the theory and practice of administration for research." Cf. Don K. Price, *Government and Science; Their Dynamic Relation in American Democracy* (New York: New York University Press, 1954), pp. 185 ff., 202, arguing for scientists in administration because of need for men with scientific knowledge plus administrative ability in solving certain kinds of policy problems. C. P. Snow urges scientists within government because they possess foresight, whereas professional (British) government administrators tend to short-term thinking. *Science and Government* (Cambridge: Harvard University Press, 1961), pp. 82f. In his Postscript, he emphasizes the need for practical judgment in scientists who give advice and the danger of having only one scientist to speak among non-scientists.

without actually making any substantive contribution to it.[26] He may treat his subordinates as only means to ends which he defines. These are the sins of pride, and temptation to them always accompanies the holding of power. The danger in scientists becoming administrators is the danger in any man's taking power; scientists, too, are corruptible.

The challenge for the scientist-turned-administrator is to nourish within his agency the search for truth, rather than for power, to foster reverence for honest science. Many a factory worker or white-collar worker finds his job hopelessly unchallenging and degrading.[27] The scientist, though he too works within bureaucracy, may find his work rewarding and his environment stimulating. Because his job is not inherently absurd, much depends upon the organization within which he works.[28] No task is harder for the research administrator than to convey to his subordinates the belief that in this agency the ideals of the good scientist still predominate, that truth is respected, that administrative and political talent is the servant, not the master, of scientific talent. Bureaucracy breeds cynicism about human purposes and about the reasons for success. The scientist-turned-administrator is challenged continuously to prove to his subordinates that justice, not expediency and chance, underlies the system. He needs humility, even "one touch of regret — not the canny substitute but the true regret from the heart," [29] by which to atone to his scientist colleagues, subordinates now, for having left research to claim the prizes of power.

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[26] For the figure of speech, and a statement by a research administrator who sensed the need for restraint, cf. Roger D. Reid, "Freedom and Finance in Research," *American Scientist,* 41 (April 1953), 286–92.

[27] Cf. Chris Argyris, *Personality and Organization; The Conflict Between System and the Individual* (New York: Harper, 1957); C. Wright Mills, *White Collar; The American Middle Classes* (New York: Oxford University Press, 1951).

[28] Cf. Committee on Engineers and Scientists for Federal Government Programs, *Summary Report of Survey of Attitudes of Scientists and Engineers in Government and Industry* (Washington, D.C.: G.P.O., 1957). "Integrity of management" seemed to be of great importance to scientists in both government and industry. Cf. Howard Baumgartel, "Leadership Style as a Variable in Research Administration," *Administrative Science Quarterly,* 2 (December 1957), 344–60. He suggests that "more effective performance and more personal satisfaction" can be attained through "participatory" leadership than through "directive" or "laissez faire" leadership. Cf. Marcson, *op. cit.,* pp. 121–44, respecting the possibilities of mixing "colleague authority" with "executive authority." For a personal insight respecting the academic parallel, cf. *From Max Weber: Essays in Sociology,* trans. H. H. Gerth and C. Wright Mills (New York: Oxford University Press, 1946), p. 134.

[29] The phrase is E. M. Forster's, for a quality in a colonial administrator which "would have made him a different man, and the British Empire a different institution." *A Passage to India* (New York: Penguin Books, 1946), p. 42.

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WILLIAM V. CONSOLAZIO

The Fiscal Dilemma of Academic Science

The technological and scientific enterprise in the United States has been doubling about every five years since World War II. Technological development now costs about $20 billion annually, of which the federal share is some $15 billion. This investment is proof of our inventive spirit and our economic discernment, but wisdom in the planning for and utilization of our scientific and technological manpower is not always evident. Major commitments which constitute long-term drains on these precious scientific resources are often made on the basis of national pride, quick return, and political expediency.

How the United States faces up to meeting the needs of academic science—faculty, facilities, and funding—will determine to a large degree whether our science and technology will remain vigorous and viable. Will we find the means to pay the costs for expanding and strengthening our academic institutions? Will we be able to train adequately all of the capable young people already committed to careers in science, mathematics, and engineering? Will academic research continue to grow and flourish as it has during the last two decades? The following discussion does not presume to answer these questions; it merely examines the nature and the magnitude of the problem and submits a model that may prove useful, even while the numbers used do not claim to show more than the order of magnitude.

• THE DILEMMA

To achieve a substantial growth in higher education in science, mathematics, and engineering over the next ten years is well within our intellectual potential and the capabilities of our economy. However, nationwide resistance appears to be developing to the increasing fiscal obligations needed for such growth. Some of this resistance is due to lack of awareness of the forces that shape higher education and academic science in our time; but most seems to be of an economic nature. Unless ways are found to loosen the purse strings of an already generous nation, irreversible educational trends may be set in motion that will endanger the country's social, political, and economic future.

U.S. colleges and universities are undergoing severe internal stresses. While providing higher quality education and expanding scientific research, educational institutions must take care of a student population that doubles every ten years. The stresses are compounded by decreasing private investment and increasing public resistance to the constantly mounting costs. The traditional sources of financing higher education and aca-

COSTS OF ACADEMIC SCIENCE AND ITS COMPONENTS IN MILLIONS OF DOLLARS

Year	*Total*	*Instruction*	*Research*	*Plant*	*Fellowships, etc.*	*Science Education*	*Science Information*
1961	$ 3,100	$ 1,400	$ 800	$ 500	$ 250	$ 70	$ 100
1966	4,900	2,000	1,100	900	500	200	200
1975	7,300	3,400	1,900	500	900	300	300
1966–75	65,000*	28,000	15,000	9,000	7,000	2,500	2,500

* Total does not add because of rounding.

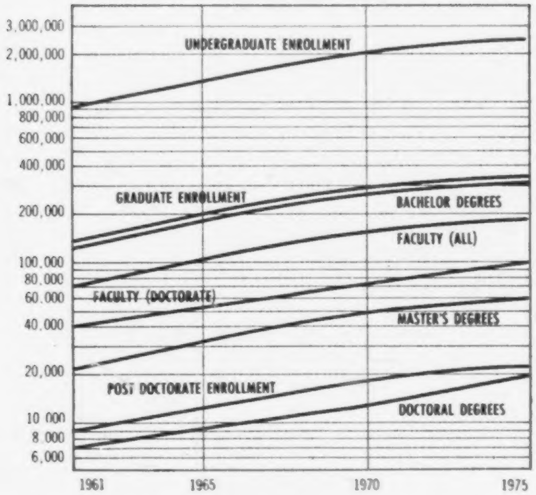

FIG. 1.—College and university science, mathematics, and engineering manpower.

demic research have been progressively giving way in recent years to a more affluent source, the federal government. The explanation is obvious. Tuition costs, at least in private institutions, have already reached a level where very few can afford them without incurring debt. Institutional endowment funds are inadequate. Philanthropic contributions are approaching an upper limit, partly because of local, state, and federal tax structures. Finally, state and local governments and their electorates often are reluctant to float new bond issues or increase taxes to meet increasing costs of education. Because of continuing national emergencies and growing needs, and because of constitutional responsibility, the federal government has steadily increased its support. There are now signs of increasing resistance to this trend.

The close relationship between science and technology, on the one hand, and education—particularly scientific and engineering education—and research at educational institutions, on the other hand, has never been properly evaluated. Recent investigations by the Edith Green Subcommittee on Education and Labor, the Carl Elliot Select Committee on Government Research, and the Emilio Daddario Subcommittee on Science, Research, and Development—all of the House of Representatives—indicate that there is need for improvement in communication, mutual candor, and understanding on the part of legislators, public officials, scientists, educators, and the public.

Most of our responsible public officials need no convincing of the material and intellectual values of science; they acknowledge national responsibility to help it grow, and yet research, particularly academic research, is usually lumped together with applied research and development and treated as frosting on the cake. The lines between research and development are too frequently blurred, leading to misinterpretation and confusion in the public mind, and this blurring is probably responsible for the increased public resistance to paying the mounting costs of academic science.

• *THE NEEDS OF ACADEMIC SCIENCE*

In a recent study, "Sustaining Academic Science, 1965–1975—a Science Resources Planning Study," I undertook to ascertain the future needs of academic science, to estimate its probable costs, and to evaluate the sources now available to sustain its quality through 1975.

"Academic science," as defined in this study, includes all research in science, mathematics, and engineering performed at colleges and universities, with the exception of research carried out in federal contract research centers and research requiring singularly expensive research facilities, such as giant accelerators. Included is undergraduate, graduate, and postgraduate education in science, mathematics, and engineering, together with activities intended to improve precollege education in science and mathematics.

In brief, the study dealt with the costs of providing faculty, research, and facilities to train the next generation of scientists and engineers. The probable cost of this educational effort was estimated by projections of recent trends in academic research and in higher education—the numerical increase in science, mathematics, and engineering faculty and student population. An assessment of the financial potential to meet these projected needs completed the agenda.

All costs were in terms of 1961 dollars. No corrections were made for the increasing sophistication of research and education and the concomitant increases in costs.

In 1975 the colleges and universities included in the sample of 900 institutions are expected to enroll 2.75 million students in science, mathematics, and engineering of whom 325,000 will be graduate students. In this same year these institutions will be graduating 300,000 individuals with baccalaureates, 55,000 with master's degrees, and 19,000 with doctoral degrees (Figure 1). To educate these individuals will require a full-time science and engineering faculty of 175,000, of whom 95,000 should hold the doctorate, or its equivalent.

The growth of academic science and engineering depends on a faculty of high quality. The latter, in turn, depends on the quality of the students, the ratio of graduates to undergraduates, the rate and quality of doctorate production, and the economic and intellectual state of the institution, as well as on the extra-institutional attractions provided by industry and government. Analyses show that 40 per cent of new doctorates will have to be fed back into colleges and universities as faculty to meet the projected growth in the next decade; in the critical years 1967–70, a feedback of some 50 per cent will be required. Under present conditions, this ratio will be difficult to attain, because of limited institutional resources and increasing employment attractions in the public and industrial sectors. A solution to the threatening "faculty gap" depends to a large degree on the federal government. The federal government, through research grants and fellowships, now supports the vast majority of research associates in science and engineering. A policy encouraging wider geographic distribution of federal aid-dependent postdoctoral associates and insuring their participation in education could increase doctoral faculty by about 25 per cent. Together with the "natural" faculty growth, this number should be adequate to satisfy the graduate faculty needs of the seventies.

To satisfy the projected growth of academic science in the decade 1966–75 will cost the nation approximately $65 billion (Figure 2)—$28 billion for instruction, $15 billion for research, $9 billion for plant and equipment, $7 billion for scholarships, fellowships, and traineeships, $2.5 billion for improving science education, and a similar amount for the maintenance of adequate communication facilities. The accompanying table shows a comparison between 1961 expenditures (estimated) and projected total cost of academic science and each of its components for the decade 1966–75.

During the decade 1966–75 academic institutions will need a "mix" in academic science funds quite different from what is now the practice. They will need a mix of approximately 43 per cent of the total for instruction, 23 per cent for research, 15 per cent for physical plant and equipment, 11 per cent in individual aid for fellows and trainees, 4 per cent for the improvement of science education, and 4 per cent for science information services. To insure adequate distribution of funds, the federal government, the principal supporter of academic science, will have to modify its science support activities, paying more than usual attention to the total needs of education in science and engineering. Insuring a proper mix of funds calls for a high order of cooperation among the various federal agencies.

At best, some $37 billion might become available, during the decade under consideration, from all traditional nonfederal sources of support: from state and local governments one might hope for $13 billion; from institutional sources (tuition and related student fees), $13 billion; from private gifts, grants, and endowment earnings, $6 billion; and from all others, $5 billion. The bulge in Figure 3 indicates the more support will be required from all sources and especially from the federal government in the years 1966–70. (In part, of course, this bulge is attributable to the post–World War II population boom, reflected in undergraduate enrollment by 1965 and in graduate enrollment by 1970.)

The residual need—the difference between costs estimated and the income anticipated from traditional nonfederal sources—will be at least $28 billion. To provide it is the federal responsibility. The annual federal commitment to academic science should be at least $1.7 billion by 1965, $2.9 billion by 1970, and $3 billion by 1975.

To meet the fiscal objectives of academic science is within the realm of possibility—but only if each of the

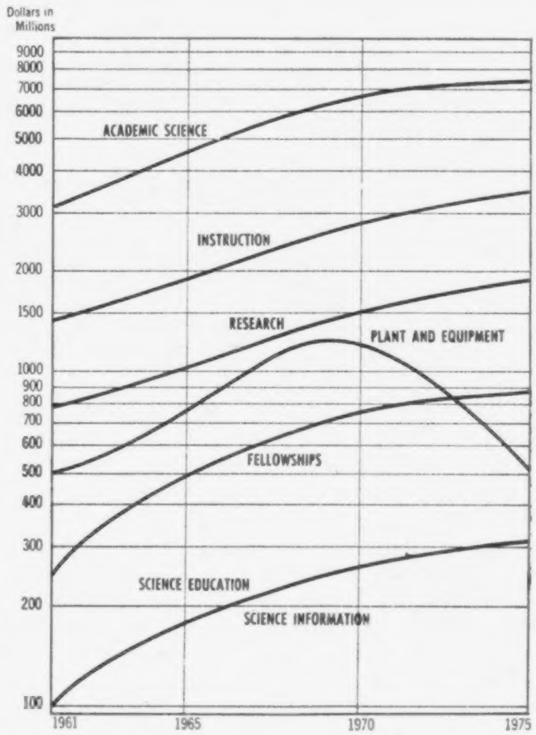

FIG. 2.—Projected costs for components of academic science

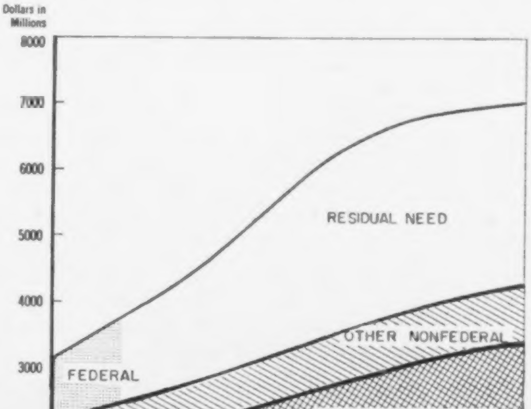

FIG. 3.—Academic science sources of support and residual need

contributing sources does provide the income estimated in Figure 3. The nation has met its financial obligations to academic science through 1963. But colleges and universities face hard times in the next ten years. Local and state government resistance to additional taxes, restraints imposed on tuition and investment income, considerable shifting in private giving from natural science to other scholarly pursuits, and increasing concern of state legislatures for undergraduate and precollege education signal grave difficulties ahead.

R. G. Fowler has indicated that state governments are becoming less responsive to the needs of graduate education. There is some logic, in this case, on the side of the states, for doctors in science and engineering are highly mobile, and this population has become a national rather than a regional resource. To assure adequate graduate training in science and engineering has thus become a federal responsibility.

• *COURSE OF ACTION*

Academic science has reasonable claims on federal funds. The U.S. Treasury is the only possible source of support of sufficient power to meet its future needs. The Seaborg Panel of the President's Science Advisory Committee gave much attention to the relationships between science and technology and academic institutions. Its 1961 report concluded that, "Whether the quantity and quality of basic research and graduate education in the United States will be adequate or inadequate depends primarily upon the government of the United States. From this responsibility the federal government has no escape. Further, it will find the policies—and the resources—which permit our universities to flourish and their duties to be adequately discharged—or no one will."

The federal government cannot escape primary responsibility for the support of academic science for the foreseeable future; nor is there an easy way out for the traditional sources of support. It is quite clear that the academic institutions themselves, philanthropies, and state and local governments must continue to carry their share. The strength and vitality of higher education in the United States is due, to a large degree, to pluralistic support and control. We cannot afford to tamper with this pattern.

Any slippage in support from any one of the present sources will lower the quality of the training students receive and will decrease the research output, leading ultimately to a decline in the nation's science and technology. Curtailment of enrollment commensurate with the limited availability of faculty and facilities seems unlikely, at least in the next ten years. The projected increase in the numbers of scientists and engineers, whether they have baccalaureates, masters, or doctoral degrees, is inevitable, for these individuals are already in the educational pipeline.

The basic questions therefore, are: Will the students now committed to careers in science and technology be trained adequately to meet the challenges offered by an urbanized, technological economy? Will the nation rise to the occasion by investing significant portions of its intellectual and material wealth in a most basic and vital national resource—the next generation of mathematicians, scientists, and engineers?

The answer to these questions must be yes. Even though the total cost of academic science projected for the decade appears staggering, it is well within the nation's means. The material and intellectual benefits accruing to the nation, to mankind, and to the individual will be adequate compensation. The nation has little choice other than to accept the increasing commitment to academic science as the better part of wisdom; it is by far the most profitable course that is open to us.

> *The planning for the identification and pursuit of technological objectives, no matter how feasible or worthy, should not be permitted to monopolize the national effort at the expense of science, and of basic science in particular. Such a policy leads in the long run to diminishing returns and ultimate stagnation. Any attempt to forecast detailed money and manpower requirements for free research in the component scientific disciplines is, in my opinion, a questionable undertaking, no matter how experienced and distinguished the reviewing body. Applied research will always receive this kind of attention. But such attempts for free research introduce a concerted extrapolational bias into the system and sound an authoritarian note. Besides, what stronger motivation can there be for creative, original research than the individual scientist's own evaluation and decision as to the most promising course for him to pursue? As history abundantly proves, the capital discoveries in science generally lie in the unknown and cannot be predicted or planned for—and these may occur in any branch of science.*
>
> —Alan Waterman
> Science, January 1965

Development of New Programs
National Institutes of Health

Program Issue Paper
August 25, 1965

The Issue

What are the likely areas of future growth and expansion in the next five years, particularly in new programs? This is the major question as posed by the Bureau of the Budget. A series of corollary questions were also raised relating to: (a) the future NIH role in institutional development, (b) future use of broader forms of institutional or research support, e.g., more program or departmental grants, (c) exploitation of industrial capacity for medical research and biomedical engineering and (d) specialized research or research service facilities. A final inquiry was directed toward the extent to which improved coordination with other parts of HEW or other agencies will be a necessary or desirable ingredient in the development of such new programs.

Background

In the twenty-year period 1945 to 1965 the National Institutes of Health has undergone a metamorphosis that has transformed its nature, role and the scope and significance of its activities. In 1945, the tangible attributes of NIH were those of a small in-house Federal laboratory. Its total budget for that year was less than $3.0 million of which only six percent went to support university scientists. Today, 1965, NIH is a complex involving nine Institutes, two major program divisions, an array of supporting activities which in total expend over $1 billion in the conduct and support of research and training and the augmentation of research facilities and resources. As such it administers the world's largest and most advanced set of laboratories and clinics engaged in medical research; its funds account for 40 percent of the national expenditures for research in the health sciences and more than one-third of Federal research funds made available to institutions of higher education.

As a consequence of this development the policies and programs of NIH exert a pervading influence upon the progress of science, the course of academic affairs and the evolution of public policy in respect to health and science. Thus this twenty-year period may be viewed as the first stage of a process of development which has brought NIH to a state of maturity, strength and preparedness for the next major phase of its evolution.

There has been a pattern in this process of past growth which is useful to review as a preface to the discussion of the factors and forces which will influence the future. This pattern reflects four periods:

1945–1950—The initial shape is formed. During this period there occurred the significant beginning events which set the stage for the course of subsequent NIH development. In 1944 the Public Health Service Act was enacted providing, for the first time, basic authority for the making of grants for research projects carried out in nonfederal establishments. The transfer of the residual OSRD university contracts in the area of the medical sciences to NIH in 1946 constituted the beginning base of the present day NIH extramural research support program. By 1950 seven of the nine research institutes which comprise the present day National Institutes of Health had been authorized and their programs launched. The total NIH budget exceeded $50 million of which over $20 million was being expended in the form of extramural grants and awards for research and training. The national commitment to a scientific attack upon the major diseases engaging the whole of the relevant research community of the Nation had been made.

1951–1955—The framework for action constructed. During this five years the growth in total appropriations was nominal reaching approximately $82 million in fiscal year 1955, an increase of only $30 million over 1950, however the increase in extramural funds was twice that of intramural. During this period the major effort was centered in forging the mechanisms, policies and procedures of extramural support; the processes of review, selection and award of grants; and the relationships with outside advisory groups which still comprise the essential framework for the administration of NIH extramural activities. The key element in this period of development was the decision, implicit in the study section review and priority rating process, to concentrate resources upon meritorious research projects emerging in the most part from the fundamental science programs of academic institutions. This reflected the early and fructiferous determination that the eventual conquest of the major diseases could only come about through advancement of the basic biomedical sciences. By the end of fiscal year 1955 the NIH was heavily involved in academic science; 80 percent of its research grant funds were going to colleges and universities. The stage was set for a major expansion.

1956–1960—The years of growth. The guiding principle of this period of NIH development was the concept that the expansion of medical research in the national interest should not be restricted by lack of funds and that the necessary resources for this expansion should either be made available or created for this purpose. This principle was initiated by Secretary Folsom in fiscal year 1956, ratified by the Bayne-Jones Report in 1958, and acted upon with vigor and swiftness by the Congress throughout this period. Between 1955 and 1960 NIH programs expanded over five-fold, reaching a level of $430 million in the latter year including construction grants. The NIH investment in the development of resources was substantially enlarged. A matching grant research facility construction program was authorized and support of training increased by a factor of seven. Large numbers of new investigators in the biomedical sciences and from adjacent disciplines were enlisted in the national medical research effort. New fields of scientific endeavor were cultivated including biophysics, mathematics and the behavioral sciences. Engagement with science on an international basis became an essential component of NIH programs. Crucial problems of public policy relating to the well-being of science, university-government relationships, and the conditions of accountability in the use of public funds began to appear on the horizon. By 1960 the period of adolescence in the development of NIH was drawing to a close.

1961–1965—The emerging maturity. In 1961 NIH appropriations exceeded one-half billions of dollars. Its programs were a significant force in the national scientific scene. The problem of stable support for the institutional base of research and training was diminished by the enactment of general research support authority and the initiation of the General Research Support Grant program in 1961. The framework for and scope of program operation was rounded out by the creation of the National Institute of General Medical Sciences and the National Institute of Child Health and Human Development in 1963 and the establishment of the Division of Research Facilities and Resources in 1962. The long-standing principles, terms and conditions guiding the conduct of the extramural program were subjected to searching examination and reassessment by virtue of Congressional inquiry out of which has emerged a more structured, articulated, and formal framework for grant administration.

During this period of rounding out and shaking down, the pace of appropriation increases was substantially slower in relative terms. Fiscal year 1965 appropriations were only two and one-half times larger than in fiscal year 1960 but the total amount now exceeds $1 billion. The pace of program activity and scientific advance, however, continued to accelerate. The early and long investment in the broad base of science now began to pay off in important enlargements of knowledge, sophisticated insights and hypotheses, and the emergence of new technology. Opportunities for more purposeful pursuit and exploitation of these advances loom clearly. The need to assure effective transfer of this capability to the diagnosis, treatment, prevention of disease and the advancement of health is becoming inescapably urgent. In the midst of these challenges and opportunities the scene of academic science is confronted with a new set of imperatives generated by the force and thrust of advancing science itself as well as the dynamics of population, economic and social change. Science stands at the threshold of the "Age of Biology" and medicine is challenged with the prospect of assimilating profound technological and professional changes. This is the setting at the beginning of fiscal year 1966 out from which NIH must gauge the nature and course of future growth.

The areas of further NIH growth

A consideration of the future course of development of the National Institutes of Health must start with a clear sense of the purpose and long-range goals which will shape the emphasis and direction of program and policy evolution. Succinctly but broadly stated, the mission of the National Institutes of Health is viewed as the advancement of the health and well-being of the American people through scientific effort. The achievement of this mission is being sought through the pursuit of the following goals:

1. Greater understanding of the biological and behavioral phenomena underlying disease, disability and health through a broad program of investigation of life processes.
2. Advancement of existing capability for the diagnosis, treatment, prevention of disease and the maintenance of health through expanded and enhanced scientific, academic, and technological efforts and resources.
3. Acceleration of the effective flow of new knowledge and technological capability from the centers of scientific and academic medicine to the universe of health practice.

Within the context of this framework of purpose and intent the real and determinant factors that will bear upon the evolvement of NIH activities and thus the future trend of medical research expenditures can be examined in the following groupings:

Intramural laboratory and clinical research. The further development of the NIH is based on the conviction that its direct research activities constitute the essential and vital scientific core of the NIH. Much of the sophistication and scientific leadership that NIH has brought to the administration of its national programs has derived from the presence, in the midst of these activities, of a center of scientific excellence and vigor. The role of NIH as an outstanding research institution pervades the Bethesda campus and provides a setting that has imperceptibly, but nonetheless profoundly, influenced the development of the whole organization. The prestige conferred on the whole of NIH by the eminence of some of its scientific staff and the solid base of experience provided by NIH's direct involvement with the leading-edge of research has won for it an acceptance—by the scientific community, by the public and by the Congress—which it could not have achieved and would not be able to maintain, as merely a government agency charged with responsibility for the disbursement of Federal grant-in-aid appropriations.

So long as the NIH grant programs were largely concerned with strengthening the national research base, the extramural and intramural activities could—and did—remain largely independent. However, due to encouraging progress in the basic sciences, more attention can be given to the deployment of effort in support of carefully selected and quite specific program purposes. The time has come when it is feasible to think, in practical terms, of a comprehensive Institute approach to each of NIH's categorical missions in which certain elements of the intramural and extramural programs can function as an inter-dependent team, while continuing to maintain a broad base of relevant fundamental research as an imperative of each Institute's program. Several Institutes—notably NCI—have already taken steps to involve a portion of their intramural staff on a formal and continuing basis in the development of carefully selected Institute programs.

The factors which will influence the scope and magnitude of the NIH intramural effort are the availability of space, the imperative demands of maintaining a position clearly in the vanguard of the advancing biomedical sciences and the increasing role and significance of intramural capability in the overall direction of Institute programs. Thus planning for the intramural research program will encompass the expansion of activities into the new facilities which will become available in the next few fiscal years, both on the main NIH reservation and at the animal farm and at locations outside the Washington metropolitan area. It is estimated that the completion of the authorized new research buildings for the National Institutes of Mental Health and of Neurology and Blindness, the National Cancer Institute, the Division of Biologics Standards, and the new Division of Computer Research and Technology, as well as vacation of space in the Clinical Center by the move of the NIH library to its new building, will expand the laboratory area on the main reservation by some 70 percent during the fiscal years 1967 and 1968. Substantial facilities at the Animal Center and the buildings for gerontology at Baltimore and perinatal research laboratory of the National Institute of Neurological Disease and Blindness in Puerto Rico will also become available in fiscal year 1968. In addition, it is planned to develop in the next five years a significant intramural research program for the National Institute of Child Health and Human Development through the use of facilities at the Naval Medical Center, collaborative efforts with the Division of Indian Health and with regional primate centers, and contractual arrangements made with universities and research institutes; much of the intramural program of this Institute will eventually be housed in a research building, planning funds for which are in the 1966 budget.

The Dynamics of Academic Science

Three-quarters of the expenditures of NIH research grant funds take place in colleges and universities. The support for academic science will continue to be a dominant area of NIH activity since the advance of the fundamental sciences will continue to be crucial to the successful engagement with the problems of disease and health. The requirements for NIH support in this area will derive directly from the intrinsic processes of growth and the cost influences affecting the development of graduate research and education and the basic academic framework of the Nation. These dynamics of growth and change in the academic scene are the consequence of a set of general but independent forces such as: the rate of population growth expressed in the moving wave of undergraduate, graduate and post-graduate students; the concommitant expansion of academic and research institutions with their additional faculty and staff demands; the upward trends in prices and wages, and the effects of advancing sophistication and technology upon the substantive costs of research. More specifically, growth and change will be determined by Federal intent expressed through resource programs, not only of NIH but also those of the Office of Education and the National Science Foundation.

A basic requirement, therefore, in the overall management of NIH programs is the evolution of a stable—but not static—support relationship to this impelling pattern of academic growth and change. The components of this support relationship will encompass:

1. A factor for increase in research project support to meet the need of new investigators and new ideas which emerge annually in the context of high standards of scientific merit, rigidly maintained.
2. A supplementation factor which can correct for the advancing economic and substantive costs of research to maintain equivalent levels of research activity through the period of committed support for research projects and programs.
3. Expansion of the General Research Support Grant program to the full support authorized by the statute and the extension of this form of support to the biomedical-related activities of university graduate schools.
4. Utilization of general research support authority to support the planned development of the biomedical research and training programs of institutions to enlarge the number of centers of excellence in the areas of science relevant to the problems of health and disease.
5. Additional support for training and fellowships at a rate reflecting the dynamics of demographic change, expanding academic capability, and faculty needs to assure at least a constant proportion of manpower flow into the biomedical fields to meet research, teaching and advanced clinical requirements.
6. Securing legislative authorization for a program of career support for research and teaching positions in the biomedical sciences to enlarge the number of full-time stable prestigious chairs for scientists and teachers in these fields.
7. Provide for the construction of research facilities and the expansion of research resources at a rate sufficient to overcome the qualitative and quantitative deficit and backlog, to meet specialized needs (e.g., animal facilities and resources) and to complement the dynamics of institutional and manpower growth.

8. Specific efforts to engender broad institutional planning to anticipate and accommodate the institutional impact of the further expansion of biomedical science, the need to stabilize graduate education in this area and the enlarging social and service demands impinging upon university medicine.

The concept of support for that segment of academic science which undergirds the program objectives of the NIH accepts as sound, and continuing the policy that the advancement of basic science is a derivative, but nontheless essential, responsibility of the mission-oriented agencies utilizing science to accomplish their politically-determined non-scientific ends. This arrangement has brought the force and urgency of social objectives to bear upon the rate of resource allocation for science. Thus, the scope and magnitude of support for academic science is undoubtedly far greater today than could have been expected on the basis of seeking support for science for its own sake. In like manner, the impetus and dimension of support for the "Age of Biology" will derive in very large part from the social goals that this expansion will serve.

The Emergence of New Science

Additional emphasis—in a purposeful and cohesive manner—will be brought to the important NIH role in surveying the developing edge of science in terms of the broad or specific implications of new phenomena, new areas of inquiry, new scientific opportunities and problems for the further development of NIH programs. The gaining of intelligence of this type would be accomplished in a variety of ways through: more concentrated and sophisticated staff efforts; greater use of NIH study section and review panels in their respective subject matter areas; the convening of special review seminars, advisory groups and scientific conferences; through the operation of information centers in specialized fields or problem areas; and through the planned commissioning of critical reviews and syntheses of the state of science in given areas.

Through these activities and assessments, areas can be identified where special effort is warranted to accelerate the scope and pace of scientific activity, to generate the specialized facilities, manpower or other resources needed and to broaden the awareness, comprehension, and extension of such advances. On this basis, the necessary planning, budgeting, and programming efforts can be carried out.

In these areas there is great opportunity and freedom to exert a major and determining influence upon the rate and direction of scientific development in relation to the goal and objectives of NIH programs. NIH is presently engaged in special efforts to influence the rate and character of research and training in such problem areas as pharmacology and toxicology, gerontology, the developmental sciences, mental retardation, the basic dental sciences, and in such new directions as bio-engineering, the extension of advanced physical and mathematical concepts to biology and medicine, and the biology of reproduction and development. These special efforts will, in turn, affect the base of academic science through the modification, expansion or redirection of research and training therein, or through the development of new institutional forms such as research institutes, to the extent that the needed effort is incompatible wth conventional academic departmental structures.

Development and Programmatic Research

The scope of potential contributions to health and medicine from advances in the physical sciences and related engineering and other technology is not fully assessed but appears likely to be impressive. These contributions range from new natural and synthetic materials, sophisticated and miniaturized instrumentation and electronic devices on the one hand to computer technology and systems analysis concepts on the other. The key factor in program development will be the determination of when a field—or a biomedical problem area—is ripe for exploitation.

In a number of areas information is at hand that will permit workable definition and specification of the nature and dynamics of biological processes both normal and pathological. With such specifications it is possible to explore the development of support or replacement systems for physiological processes and organs and to pursue in a deliberate and programmed manner specific diagnostic and therapeutic approaches to certain disease problems.

The NIH engagement in this area is already of a distinct and growing dimension:

—The cancer chemotherapy program, the oldest and largest

—The special virus-leukemia research program

—The vaccine development program

—The psychopharmacology program.

New and potentially substantial program efforts are in the beginning stages:

—The artificial heart-artificial kidney programs

—Advanced instrumentation (e.g., ultra-centrifugation) and specialized research environments (e.g., "life islands" and virus containment facilities)

—Automation of analytical chemistry and other clinical and research laboratory processes

—Computer applications to communications, hospital systems, biological simulation and model construction

—Telemetry, monitoring systems and diagnostic aides.

These efforts present a substantial new dimension in the further development of NIH programs. They will be reflected in an increasing proportion of NIH funds expended for development, applied and programmatic research activities. They will also generate new managerial problems.

For the most part the entire present framework of the National Institutes of Health for administering its support programs in the area of research has been developed in the context of the research grant as the principal instrument for support and the community of academic and nonprofit research institutions as the principal resource for scientific effort. *In the administration of development pro-*

grams the contract will be the principal instrument for support of scientific and technical activity and, in a very large part, the source of scientific and technical capability will be private industry. In addition, the nature of development activity, involving as it does clear specification and control over the conduct of the scientific and technical activity being carried out, *imposes different demands upon the administering organization.* Thus, NIH is confronted with the task of bringing *into being a more comprehensive and sophisticated framework of policies, procedures, and operating mechanisms relevant to the conditions of contracting with industrial research institutions.*

The development of such a framework will present particular problems in providing for appropriate access to advice in establishing the mechanisms and criteria for qualitative review and selection and in designing the specific arrangements for the management of contracts and projects. In this respect the National Institutes of Health will expect to draw heavily upon the experience of the Department of Defense and other Federal agencies which have been engaged in large-scale contract programs for R & D utilizing industrial capability. Discussions between such agencies and the staff of the National Institutes of Health have been initiated as a basis for designing organizational and managerial development arrangements.

New Insights into Disease

The characteristics of disease and disability, and of the human mass affected by them, are diverse, complex and changing. Thus, knowledge gained and hypotheses formulated on the basis of observations in the past may lose relevance in current and future situations. Therefore, these characteristics and the dynamics of their change consitute a vital set of phenomena to be studied for their bearing upon problems of health and disease.

The rapidity and complexity of current changes taking place within the population mass of this country and the world represent a metamorphosis with profound import for the further course of medical research. These changes encompass:

1. Changing natality and mortality rates and patterns and the consequent acceleration in population growth
2. Changing genetic patterns and composition of population
3. The shift in age distribution
4. Changing economic and social characteristics
5. The mobility of populations and the changing patterns of their geographic and environmental distribution.

All these factors individually and collectively have had, and will have, both distinct, subtle and profound influences upon health and disease.

To some extent, emphasis and direction of effort in the medical sciences derive from a series of general notions about population characteristics in relation to health and disease. The prospect of far-reaching changes in demographic phenomena and the associated changes in disease patterns have made this an increasingly important area of scientific investigation. The possibility for searching study of these problems is strengthened by the growing technical capability (via new statistical theory, information theory and computers) to elicit systematic and meaningful information from large masses of data encompassing large numbers of complex variables. Thus, further growth is anticipated in biometrical, epidemiological, and demographic studies in the major NIH research programs.

The Extension of Academic Science to the Universe of Health Practice

The extent to which the knowledge, capability, and new technology emerging from research is being brought into effective use in the clinical and health service scene has been a matter of acute concern for some period of time. Strenuous and somewhat tumultuous efforts to demonstrate progress in this area under the amorphous label of "communications" has been a dominant characteristic of the recent national scene. Much of this activity distracted from rather than contributed to understanding the true nature of this problem and the development of realistic approaches to its solution.

The event that made it possible to break loose from the limiting and diffuse array of "communications" efforts was the release of the Report of the President's Commission on Heart Disease, Cancer, and Stroke. The innovative concepts of this report, providing as they do for the direct linkage of the centers of scientific and academic medicine with community hospitals and the framework of community medical services, bridge the critical gaps in what potentially may be the most effective and meaningful manner. In a very real sense the parallel of the extension service which has served so successfully to bring the knowledge and technology generated in agricultural research schools into swift and effective application in agricultural practice and marketing is now being adapted to the scene of medicine through extending the functions of academic medicine and their relationship with community medical services.

The legislation which incorporates these recommendations (S. 596 and H.R. 3140) now before the Congress provides for both planning and operational grants leading to the establishment of medical complexes on a regional basis throughout the country. Each medical complex would encompass a regional medical center having major teaching functions, one or more specialized research centers and one or more diagnostic and treatment stations. These entities would be linked by exchange of staffs, integrated research and training activities, programs of continuing education and demonstration which would reach from the private physician and his patients on the one hand to the advanced research, training and clinical programs of a university medical center on the other. These arrangements would provide for the direct and expeditious flow of knowledge, capability and technology outward from the centers of academic and scientific medicine and the speedy referral of patients for specialized diagnostic, therapeutic and research purposes inward from community medical care framework.

These concepts reflect an elegant simplicity and rightness which, if this legislation is enacted and it is indeed possible to carry them into practice, will undoubtedly exert a pervading influence over the character and quality of medical care available to the victims of these dread diseases.

The decision by the Surgeon General to place the administrative responsibility for this program at the National Institutes of Health will add a major new dimension to its functions and responsibilities. The potential magnitude of expansion is indicated by the size of the appropriation authorization added by the Senate in its passage of S. 596. This authorization reaches a level of $400 million in the fourth year of program operation.

The administration of this program will generate additional organizational and operational complexities for NIH. A new operating division along with its associated advisory council will have to be created and staffed; regulations, operating criteria and procedures developed and promulgated; the coordination of program and operating relationships with the categorical Institutes, the relevant programs of the Bureau of State Services-Community Health and the Vocational Rehabilitation Administration worked out.

Conclusion

Looking to the future along the lines of growth examined above, it is clear that the task of adjusting NIH managerial and program direction capabilities to the demands of these developments will be substantial. A first order of action will be the enlargement and redirection of the program planning functions both in the Office of the Director and in the several operating programs. A new activity structure for the development and execution of the budget reflective of these mainstreams of growth and program decision will be required.

The nature of this contemplated program growth will *shift the center of gravity of NIH administration from dominant concern with grants management to areas in which Institute and program decisions will be the prime determinants of the direction of efforts.* These will involve, for example, the choice of new areas of science for emphasis; the determination of the practical feasibility of undertaking major development action or the selection of areas for intensive programmatic research efforts; the design of large scale field studies. Program direction and management of this kind will place a high premium upon scientific and technical judgment, managerial skill, planning and evaluative capabilities. Certain organizational changes to provide for the most effective discharge of these tasks may be required. *The demand for high managerial and technical competency* in program staff will *place increasing pressure upon the crucial obstacles of salary and position levels.*

As noted throughout the discussion of program growth, extensive and continuing relationships with other Federal agencies within and without HEW is contemplated and will be maintained. Joint planning arrangements amongst the National Science Foundation, the Office of Education and the National Institutes of Health have already been initiated and liaison with the Office of Science and Technology is already a well-established practice.

At the end of the next five years the National Institutes of Health may well reflect the order of change and increase that occurred in the 1956–1960 period. The task will be to make certain that the strength and promise of that youth is formed into a sound, capable, well-balanced and effective maturity.

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Academic Science and the Federal Government

Philip Handler

Responsibility for the welfare of American science with commensurate financial support of research and education in science is a recently established role of the federal government. The rapid growth of this endeavor has occasioned numerous searching inquiries by the executive and legislative branches of the government, by the academic community, and by the press. A growing literature reflects deepening concern with the relationship between science and society, and seeks to develop an appropriate base in philosophy and understanding to guide those responsible for government science policy. This article is intended to provide a more immediate focus for some aspects of this discussion.

Although the Constitutional Convention of 1787 explicitly rejected efforts to grant the federal government constitutional authority for the pursuit of scientific inquiry, over the course of the next sesquicentury that government found itself increasingly involved with science and technology. Nevertheless, before World War II, except for federal support of the state agricultural experiment stations and the highly selective actions of a few philanthropic foundations, research was largely financed from the meager operating funds of those institutions in which it was conducted, that is, universities, a few research institutes, and government laboratories. After the war, augmented support for basic research was provided from funds which, in the American tradition, had been collected or appropriated to further distinctly applied missions, such as a hoped-for cure of cancer or a new weapons system. Private support, particularly of biomedical research, increased greatly, but, by the mid-fifties, the federal government had been established as the major patron of science in our country.

The Office of Naval Research embarked upon an enlightened course of programs for support of research in almost all areas of science. The National Science Foundation was charged with assuring the vitality of American science. As its appropriations increased, the Foundation developed a panoply of individual programs in support of research, science education, and scientific information. Withal, NSF is today responsible for only 15 percent of federal support of research at academic institutions proper. The National Institutes of Health multiplied and, by means of a diversity of programs in support of biomedical research and research training, transformed the nation's medical schools while also strikingly upgrading many departments of biology and chemistry. New agencies, organized to manage exceptionally large enterprises—exploitation of the potential of nuclear energy and the exploration of space—also found it useful to engage the academy in their programs, while the other military services followed the earlier lead of ONR. Occasionally, proponents of a Department of Science appeared, but their proposals were rejected and, instead, there evolved a pluralistic pattern of support not only of specifically mission-oriented research, but of fundamental research at the frontiers as well.

By the historical accident that a pre-

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ponderance of the nation's most competent scientists were on university faculties at the time of World War II, and no tradition of government-supported research institutes had previously been established, universities, collectively, became the seat of the major thrust of fundamental research in the United States, a pattern strikingly different from that of many European nations, most notably the Soviet Union. This unplanned development engendered a uniquely successful system of graduate education within which education and the conduct of research are indistinguishable: young scientists are given meaningful research opportunity at what may be the most productive stage of their careers; and the endeavor occurs within coherent departments which are frequently of a magnitude sufficient to exceed the minimal critical intellectual mass essential for success. The enterprise nurtures not only the few highly talented young people who will become tomorrow's scientific leaders, but also the much larger numbers of scientists needed to staff our educational institutions, government and industrial laboratories and, in so doing, take full advantage of the accomplishments of those highly talented few. This may well be the prime basis for the "technology gap" between the United States and those European nations which contribute their share of effective, highly talented scientists, but fail adequately to capitalize on their contributions. The patent success of this educational endeavor seems all the more remarkable since it has been accomplished, largely, by funding mechanisms designed to support research qua research, rather than graduate education, and is administered by federal agencies which are charged with a diversity of "practical" missions. But the rapid growth of this research-education enterprise has seriously stressed the universities. And it is not surprising that a system designed to purchase research results, albeit with the understanding that their application may lie in the relatively distant future, is not entirely satisfactory as the primary financial pillar of graduate education.

Political Setting

Unfortunately, during this period of growth, the academic scientific community failed to communicate to the public the integral nature of graduate education and the research process. While the press, understandably, publicized the occasional peaks called "breakthroughs," there was no equivalent effort to make explicit the manner in which research findings combine to form the mosaic which is the corpus of science and which contributes continuingly to applied research and development. Hence, it is entirely understandable that public sentiment currently urges a rationalization of the nature and magnitude of the academic research endeavor and its place in American society. Public appreciation of remarkable technological achievements in fields such as space, weapons, communications, and computer development engendered confidence that federal research programs could also ameliorate some of the more pressing problems of American society. As the term "fundamental research" latterly assumed less generous nuances of meaning in the public ear, such alternate motifs as "education," "regional development," and "equitable distribution of federal funds" became increasingly attractive on the federal scene. And there is a growing opinion that there is a relationship between the economy of a given region and the quality and magnitude of the university science endeavor within that region.

From many quarters came demands for upgrading the quality of American education at all levels and in all regions, finally tumbling the traditional barriers to federal aid to education. Without national debate, there is increasing feeling that the nation should provide every student with access to the maximum level of education which he can successfully achieve. (As a subtle consequence, whereas previous projections concerning the growth of the academic research endeavor were made in terms of the numbers of competent scientists who could be envisioned at their benches at some future date, it has now become more expedient to base such projections on estimates of graduate student enrollment at that date.)

The current decline in the rate of growth of national expenditures for science is serious indeed. Most critically affected will be young scientists fresh from postdoctoral experience and eager to try their fledgling scientific wings, particularly those who have been attracted to emerging young institutions. Although indicators of this difficulty are already evident, it is not yet maximal since there is a substantial lag period between passage of congressional appropriation bills and their impact on the distribution of grants and contracts. In some degree, the currently diminished growth rate of federal funding of fundamental research has been compensated by provision of funds ostensibly in support of education and by the, as yet, lesser effort to encourage the upgrading of the scientific endeavor in the so-called middle universities as well as in institutions of higher education generally.

When, however, the Vietnamese episode terminates, it is possible that funds on a scale larger than any in history can be made available for the support of fundamental research, graduate education, and the institutions in which these are conducted. It is imperative that at that time, the nation be armed with long-range plans based on a debated and understood philosophy and defined goals.

Limited Partnership between Government and the University

Perhaps the most important lesson to be drawn from the immediate past is that our nation should continue to capitalize on the mutually beneficial relations of graduate education and research. The support of university-based research in the natural and social sciences simultaneously and indivisibly serves diverse purposes which are of equivalent value to society. The funds so utilized make possible the education of those who will be tomorrow's teachers, investigators, and administrators; they expand the frontiers of man's understanding of himself, his society, and of the universe, while providing scientific bases both for tomorrow's technology and, hopefully, for tomorrow's social forms; and the very endeavor itself establishes the tone and quality of life, not only for those immediately at the university or in the region about it, but for the nation at large.

These may appear to be self-evident truths, but this unitarian doctrine has not been universally accepted. Witness the pretext that graduate instruction and the research endeavor of the university are separable entities for bookkeeping purposes. This is expressed in the guidelines for "effort-reporting" associated with federal grants and contracts, an arrangement wherein each investigator-teacher is required, periodically, to report the fraction of his "time or effort" devoted to the research supported by each specific research grant or contract. This intrinsically farcical contrivance arose from the limited nature of the partnership between government and the university. In the law, the mission-oriented agencies may have no commitment to the welfare of the university per se. Hence, it became logical to expect that, if the salary of the investigator-teacher is to be defrayed, in whole or in part, by an agency which supports his research but may bear no responsibility for his teaching function, then that agency must assure itself that the extent to which the investigator-teacher has contributed to each of his sponsored research projects is commensurate with salary payments from the related grants or contracts. Since neither the intensity nor the quality of this contribution is quantifiable, the units of contribution, willy-nilly, must be reckoned in *hours*.

On its side, the university entered into this arrangement because by this means the university can look to the federal government for a contribution to its operating budget. The brute fact is that universities, particularly the private universities, lack the funds to meet the costs of the multitude of functions expected of the multiversity in contemporary society. Today, increasing numbers of well-prepared undergraduates arrive on campus with expectations of personal encounter with faculty minds, expectations which surpass those held by graduate students only yesterday. This worthy challenge can be met only by a commensurate increase in the numbers and quality of the faculty and, hence, in the budget. The commitment of the university to graduate education, inherently the most expensive form of education since it is essentially tutorial, has grown as increasing fractions of undergraduate classes have accepted the proposition that graduate or professional education is almost imperative to life in an ever more complex society. But the university offers community services well below cost, frequently gratuitously, while undergraduate education is made available at perhaps one-third of true cost, and graduate and professional education for lesser fractions still. Indeed, since the university typically "recovers" 70 to 95 percent of the true costs of federally supported research, despite the overall magnitude of this effort, it may have become, *financially*, the least painful of the university's several increasingly expensive roles. And neither tuition nor private giving is likely to alleviate this situation.

On every university campus, while responsible educators insist that undergraduate and graduate education were never as good or as intensive as today, deficits rise and some institutions approach insolvency. If, as the more vocal undergraduates insist, educational programs are nevertheless, inadequate, the problem is not so much that universities are unresponsive or conservative—they are and should be deliberate in the face of pressures to change—but that they lack the resources with which to respond as they might wish.

Confronted with these urgencies, the universities have turned to federal funds, appropriated in support of research, for assistance in the payment of faculty salaries. When this logically gave rise to the practice of effort-reporting, the university professor, working full time, was caught in a trap not of his making and asked to submit a statement concerning the extent to which he has given of himself to but one aspect of his profession. Aware of the intrinsic impossibility of a meaningful reply, teacher-investigators in every university have protested.

But the problem is not how to keep books of account for professorial salaries. Rather the real questions are, "Are American private universities, presently responsible for about one-third of undergraduate education and a yet larger share of graduate education and research, worth salvaging? Is the private university sufficiently valuable to American society as to warrant direct subvention? Should the federal tax base be utilized for large-scale contribution to the general operating funds of both public and private universities? If so, by what mechanisms?" Effort-reporting is no less repugnant to the faculties of public universities, but it is the problems of private universities, in the main, which resulted in current practices. However these questions are answered, it is imperative that professorial salaries be removed from the project research grants system and that the requisite funds be conveyed to the universities in a manner which is supportive rather than destructive of the morale of those whose creativity, insight, and understanding form the keystone of the entire education and research enterprise.

Indirect Costs, Cost-Sharing, and Effort-Reporting

Three major changes in the management of research grants programs were initiated almost concurrently.

1) In response to repeated requests by university presidents and business administrators, Congress acquiesced to the principle of payment of full indirect costs in association with research grants funded by the National Institutes of Health and the National Science Foundation.

2) Simultaneously, Congress enunciated the principle of cost-sharing and established as equitable a distribution in which the university would bear at least 5 percent of the sum of direct and indirect costs. Since this principle had the effect of negating, in part, the consequences to the university of the former action, the universities were quick to recognize that the position could be recovered if they were to request, in applications for research grants, an increasing fraction of the associated professorial salaries. However, few took full advantage of this opportunity on the scale legally possible. The agencies were thus spared an embarrassment since Congress had not also provided equivalent additional funds; otherwise the net result would have been an absolute decrease in funds available for the conduct of research, per se. In any case, it soon evolved that the principal financial contribution that the universities could offer, in token of cost-sharing, was some fraction of the associated faculty salaries.

3) The practice of effort-reporting was initiated, in part for the reasons stated earlier, and in part out of the alleged necessity for formal demonstration of the extent to which the university participates in cost-sharing. In those instances in which the investigator actually is engaged full-time on the research project, this occasions relatively little pain. When the investigator-teacher's salary is defrayed entirely from university sources, the amount of "effort" required to satisfy the cost-sharing principle is almost invariably less than the actual case; whereas he may safely certify this contribution with complete honesty, he is irate when asked to account to the government for work for which he was paid by the university. Ironically, very few academic scientists have protested when asked, in applications for research

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funds, to estimate in advance their expected effort contributions! When, however, the university requests the maximum fraction of the investigator's salary possible under the cost-sharing formula, to the auditor it can be a matter of considerable moment whether the investigator contributed 40 or 60 percent of his effort to the project in question, whereas the latter cannot reasonably be expected to know the difference.

In the minds of many, these three independent concepts and practices have blurred into one and this has led to vigorous attacks on the principle of cost-sharing. For example, the National Association of State Colleges and Universities and the National Association of State University and Land-Grant Colleges requested an end to the cost-sharing principle and proposed that the federal government should defray the full costs of academic research including the full pro rata fraction of the professor's salary attributable to his sponsored research. To be sure, serious inequities have arisen in the administration of the cost-sharing principle, but these have resulted largely from failure to reckon the transactions between an agency and an institution colligatively instead of individually; that is, the institution may not compensate for undercost-sharing in one grant by overcost-sharing in another.

Abandonment of the cost-sharing principle could plant the foot of the university on the highway to disaster. Current accounting and reporting mechanisms and the granting instruments themselves may well be inappropriate —but retention of the cost-sharing principle is essential to assure the independence of the university, particularly the private university. To do other is to accept the notion that the government purchases from the university research which the government wishes to have performed, whereas they are and should be joined by the mutuality of their interests and the transaction should occur in the spirit of a grant-in-aid. A true university must view the conduct of research as an integral aspect both of graduate education and of its responsibility to society. Full payment by the federal government of research costs including professorial salaries is a denial of that concept and could constitute the first major step along a trail by which it would become a federal university. This may appear to some to be a desirable or realistic goal; but the trail should not be broken until it is clear that the goal has been accepted. If, indeed, there is merit in the survival of the private or state university, albeit in loose partnership with the federal government, then American society must invent new means by which these institutions are to be sustained.

I urge that the university should be enabled to meet the faculty payroll and provide all those services currently reckoned as "indirect costs" from otherwise uncategorized funds, regardless of their source, even if this is large-scale direct subvention by the federal government. Then it would be free to engage, in equivalent proportion, in cost-sharing in its undergraduate, graduate, and professional education programs, its community services, as well as in its research endeavor. Were the direct subvention ample, the problems of cost-sharing and faculty effort-reporting would be automatically eliminated. To be sure, payment of full direct and indirect costs by federal agencies, on a grant-by-grant basis, in extension of current practices would lighten the financial burden on universities. But as long as individual project grants and contracts, particularly those from the mission-agencies, include payments for faculty salaries, time- or effort-reporting will necessarily continue, as will the continuing erosion of the allegiance of the faculty to the university and the all-too-frequent disavowal, by the university administration, of responsibility for its research endeavor. Elimination of these trying problems will require drastic revamping of research and university support mechanisms and concerted action by all federal agencies, probably including designation of one agency as "principal federal agent" for a program of university subvention either by a minimum program of grants for payment of faculty salaries or, hopefully, a generous formula based on student enrollments, and the magnitude of the institution's research enterprise which more adequately permits the university to function as our society demands.

Summer Salaries

Early in the history of the federal grants programs, sanction was given to payment of the investigator's salary, on a pro rata basis, for that portion of a calendar year which is not included in the academic year. Most frequently, this has meant a 2/9 increment above the academic year salary. The practice was adopted before academic salaries had risen in keeping with the general postwar inflation and was intended to permit the investigator-teacher to continue his research rather than necessarily utilize the summer as an opportunity to earn additional income in a nonacademic setting. This course was enormously successful and, indeed, also contributed significantly to the professionalization of academic research.

But once such a privilege has been extended to one investigator, it cannot be witheld from others. In time, such supplements became the norm and were so accepted and expected by academic administrators in preparation of their budgets and pay scales. Concomitantly, academic salaries increased significantly, thereby posing a serious problem to the agencies. A supplement which helps assure a minimum decent income is readily understood. But is there some ceiling which is equally logical? How should an agency react to a request for a 22-percent supplement to an academic salary which exceeds the 12-month salary of the agency head? Should the agencies consider some maximal annual total rate, or some maximum rate of summer payment? Patently, any such modification of the present arrangement is a step toward federal establishment of academic salaries. And again, a rational solution requires federal subvention of the university rather than of individual investigators through the project grant system.

Institutional Development

Pressure to upgrade the scientific activities of universities which are not currently in the very front rank arises from the generally enhanced expectation of Americans everywhere with respect to the quality of life and from the belief that the science endeavor at a university contributes significantly to the life and economy of a region. This was certainly the role of the agricultural schools of the land-grant universities in the flowering of the agricultural revolution; it should be true of all universities in the scientific revolution.

Economists are divided on the extent to which regional economic vigor reflects the scientific quality of its focal university; indeed some argue the converse, that it is the vigorous economy which supports the great university. But there are examples of successful stimulation of the life and economy of a region by deliberately capitalizing on its university base. The proximity of North Carolina, North Carolina State, and Duke universities, "middle universities" all, prompted the development called the Research Triangle. This attracted substantial industrial and federal laboratories; in turn, these attracted others and the community benefits by the second and third harmonics of this activity, ultimately including better shops, better bookstores, more theatre, and so on. All increased the local tax base, giving rise to better community services and better schools, while attracting yet more industry and industrial research, and the sum of these, in turn, strengthens the universities. Such a growth cycle is not the automatic consequence of the presence of the university: it must be fostered by vigorous community effort. But it can be done where the university is sufficiently strong. How many such developments the country can successfully undertake in the next 5 or 10 years remains to be determined.

The scale and pace of attempts to upgrade universities across the country require careful analysis and planning. The process must be incremental and the pace must be set by the availability of the necessary students, scientists, physicians, and engineers, avoiding actions which might diminish the quality of science at the already established institutions; indeed, the latter must continue to progress if they are not to retrogress. To be successful, this effort will require substantial federal funds and entail individual actions made on a scale substantially greater than that possible in the present programs of NSF and NIH. Careful planning at national, regional, and state levels must resolve the recurring problem of whether to assist relatively large numbers of institutions, in small increments, or to attempt truly major measures at a few, carefully selected institutions. In either case, success of the undertaking may be estimated by the subsequent success of the faculties of these institutions in competing in the national research grants system. Meanwhile, those engaged in such bootstrap operations must cultivate patience. To cite the report of a committee of university presidents, "While other activities of the state may be improved by rapid administrative reform, the university must build its greatness and efficiency over decades."

As junior colleges and universities increase in number and size, the traditional 4-year college finds itself in a difficult plight. Most serious is its increasing difficulty in attracting faculty of the desired caliber. No simple solution is obvious and it is unclear whether, in the future, the isolated 4-year college can remain a viable organism in our society. But the transformation of formerly admirable, relatively small, private liberal arts colleges into third-rate universities by inauguration of inadequate programs of graduate education is surely an error to be avoided, if only because launching a good university is more readily accomplished than is improvement of an inadequate university. Equally thorny is the increasingly frequent problem of the state college which, having attained a large enrollment, aspires to become a graduate university although its faculty is not commensurate with the demands of graduate education. State educational planning boards would be well advised to avoid this snare by early identification of those institutions to be designated as graduate universities.

In contrast, there are some federal contract research centers and in-house federal laboratories which can boast of research staffs at least equal in caliber to those of most universities and which are engaged in research much of which does not differ in character from the research which is normal in the programs of graduate universities. Several of these laboratories contribute significantly as training grounds for postdoctoral fellows; none are thoroughly exploited as resources for graduate education. As the existing network of universities becomes saturated and as the demand for graduate education becomes more pressing, it will become urgent that the resources of these laboratories, their facilities, and their scientists become more fully engaged in the educational enterprise.

Funding Patterns

It is imperative that we preserve the patent merits of the project grant system while developing mechanisms for the support of science and science education by the transfer of funds in larger amounts than are customary today. As a governing principle, funds for a given purpose should be made available to the largest unit concerning which a qualified group of external referees can make an appropriate quality judgment.

1) Unusually large facilities which serve the national scientific community, whether radio telescopes, accelerators, or sociological data banks, and so on, are most appropriately managed by consortia of universities, in-house federal laboratories, or single universities serving as federal agents.

2) Grants for general university subvention, blocks of faculty salaries, construction, libraries, large computer centers, institutional science development, shops, animal or other large special facilities should be conveyed to the university president or the appropriate dean.

3) Block grants to provide, *inter alia*, stipends for graduate students, general research services and the research expenses of junior members of the faculty should be made available to department chairmen or their equivalent. Instead of the widespread practice of supporting graduate research assistants with stipends derived from research grants made to their mentors or expanding current federal competitive fellowship programs by more than an order of magnitude, graduate students should be supported almost entirely from such departmental grants, an extension of the concept established in the present insufficiently funded training grants of NIH and NSF.

4) Funds appropriate to the unique requirements of the individual investigator should be awarded in his name, after assessment by the now traditional peer-judgment system. Most of the other grant mechanisms should rest on assessment of the collective ability of the applicant group in question (a department, school, or university) which, in turn, must generally be the aggregate of quality judgments concerning individuals. There have been frequent expressions of doubt that such a system can remain manageable. If, for example, 15 years hence the number of eligible academic investigators will have trebled, federal agencies must adequately operate a project grant system thrice the size of the present. Nevertheless, this mode of operation is the only means of ensuring, safeguarding, and estimating the quality of these endeavors and it fully warrants whatever efforts will be required.

5) Although some postdoctoral fellows might continue to derive their stipends from research grants, funding through the departmental grants or through an enlarged national fellowship program is much to be desired. In any case, there should be explicit recognition of the fact that it is in the career interest of young investigators, at this stage, to engage in a significant amount of formal teaching. Frequently, this could be done usefully in the very institution in which they are gathering further research experience. Alternatively, there would be great advantage in having these eager and almost completely trained research scientists serve teaching internships at liberal arts colleges or junior colleges within commuting distance. Such experience would be enormously beneficial to apprentice teacher-scientists while ameliorating the current plight of these colleges.

Support of Academic Research by Mission-Oriented Agencies

Of the financial support of fundamental research by the federal government at "educational institutions proper," 85 percent is currently justified in agency budgets by its underlying relevance to their practical missions. Most of public giving, for biomedical research for example, is similarly oriented. But the academic institutions in which research is performed are not equivalently mission-oriented; universities are organized in terms of their educational functions. Accordingly, there is a mismatch between the requirements for success in much of mission-oriented research and the disciplinary structure of the university. Whereas the historical unit of academic research activity is the professor and his coterie of students, fellows, and technicians, successful "directed" mission-oriented research increasingly demands the concerted effort of a multidisciplinary team. Attempts to finance academic research by addressing the specific problems of the mission-oriented agencies could distort the intellectual structure of the university, and pose a threat both to the pursuit of imaginative independent research and to the education of the very scientists required to man such multidisciplinary teams. Hence, most research in academic disciplinary departments, regardless of the source of supporting funds, should continue to consist of individual efforts. In practice, this would probably mean that NSF and NIH would contribute the bulk of the federal funds in support of academic research. Since the leading edges of science are frequently at the interfaces between disciplines, such a policy should not be so misapplied as to deter spontaneous alliances arising out of the mutual scientific interests of faculty members. Concomitantly, however, the university might well encourage the parallel development, on campus or in reasonable proximity, of appropriately organized contract centers for mission-oriented directed or applied research. Such an arrangement could safeguard to the mission agencies the principal advantage of current practice which is frequent contact between agency scientists and those of academia, thereby helping to keep the former *au courant* and occasionally interesting the latter in a fundamental problem of relevance to the agency mission.

On the Magnitude of the National Scientific Endeavor

If one accepts as a national goal provision of the maximum education for which each student is qualified, then the number of prospective graduate students becomes a valid criterion for projection of future budgets. Our national history indicates that there have always been those who opposed an extension of the educational system—yet each such extension resulted in an expanded economy and improvement in the quality of life for the nation generally. There is no apparent reason for limiting this process and it is doubtful that the question, "Who will employ all those scientists?" is meaningful. Rather, it seems likely that, in this instance, supply engenders demand. And if, one day, a few more teachers with Ph.D.'s are found on the faculties of high schools, this scarcely seems objectionable. Meanwhile, there is a need to consider development of programs leading to an advanced degree without the requirement for a significant experience in independent research.

Unless our patterns of national life change drastically, the projections of future graduate enrollments by the Office of Education indicate a doubling of the present graduate student population by about 1976. If these projections are borne out, in a general way, they establish the future minimal dimensions of the graduate education–research enterprise. In this light, the frequently cited proposal of a gross increase in funding of about 15 percent per year for university-based science seems a reasonable match to opportunity and need which allows for the growth of the graduate population, for the cost of increased sophistication of research itself, and provides a margin for inflation, but probably seriously underestimates future costs of computer usage. Although an annual 15-percent increment affords opportunity for many types of specialized undertaking, it should be regarded as an umbrella beneath which lies, in the main, the aggregate of "small science." Specific opportunities which will require large-scale capitalization and operational costs will undoubtedly present themselves; if these are justified on scientific grounds, they need not be restricted in any one year to that which is possible within the umbrella.

Obviously, it will be necessary to construct a physical plant commensurate with such growth. In view of the long lead time required, current levels of funding for this purpose are seriously inadequate, and each year we fall further behind. Moreover, federal agencies and Congress must agree to more generous matching formulae than those in current use if these goals are to be attained.

It is no longer necessary to persuade either the public or its elected representatives that federal support of fundamental research is, in principle, in the national interest. But it will ever be necessary to justify the *size* of that effort. Surely, a central parameter for estimating the magnitude of the academic research–graduate education component must be the dimensions of the graduate student population. Federal expenditures for fundamental research outside the academic setting should be determined by the continuing needs of the mission agencies, and justified accordingly.

As this growth proceeds, it will give impetus to the strengthening of academic science across the country. Although the size of an individual university knows no fixed maximum, it probably does have an optimum. As the established institutions become saturated, well-qualified students will, increasingly, seek graduate and postdoctoral experience elsewhere—at the scientifically lesser universities already in being, or at new universities, particularly the new urban public universities. This driving force will generate the opportunity to upgrade the science activities of these universities and they should be provided with the requisite resources in faculty and physical plant. Although, admittedly, this is a painstaking, slow process, it must certainly represent the most effective, rational means to achieve "more equitable geographic distribution of federal funds."

Sources of Funds for the National Science Enterprise

In view of the broad impact of science on all aspects of society, of the magnitude of the enterprise, of the institutionalized forms of science, and the intrinsic cost of individual research projects, it seems unlikely that the role of the federal government as the major patron of science will be challenged in the foreseeable future. Even the minimum unit package of support has become a sum so substantial that few other potential sources may be seriously considered. This prospect is also evident from the fact that the nation's largest philanthropic foundations have abandoned to the public purse the support of this vital enterprise. If the general grant philosophy presented above is to be implemented, a serious challenge will be posed to the pluralistic support mechanisms of the moment, particularly to the roles of the mission agencies on the academic scene. Inevitably, the National Science Foundation, the Endowment for the Humanities, and the Office of Education must assume ever larger shares of the responsibility for academic research-education and for the welfare of academic institutions, while a special role is reserved to the National Institutes of Health in the field of biomedical research-education. Indeed, although the time is not yet at hand, it appears to be increasingly logical to consider regrouping these agencies into a single Department of Science and Education.

Meanwhile, the other mission agencies should foster specific centers for relevant fundamental research, associated with universities, rather than broad institutional programs directed at academia. The fact that agencies such as DOD, AEC, and NASA require large numbers of trained scientists and engineers, and have large total budgets, should no longer be used as an argument in favor of their support of graduate education, broadly conceived. The same Congress that votes their budgets can also provide direct support of graduate education–academic research in its own right in the budget of an appropriate agency. However, all agencies should develop uniform guidelines and minimize the number of individual types of programs. The present federal grants structure evolved rapidly as the consequence of many actions taken by both the Congress and the Agency administrators. This structure has repeatedly been altered or extended by imaginative bureaucrats who have frequently been more perceptive of academic needs and more zealously mindful of academic autonomy than have those in the universities. But now that the federal government has accepted responsibility in large part for the science–graduate education endeavor, programs for its support should be relatively few in number, simple, and forthright.

When our nation again knows peace, the academic research endeavor may hope to find stable and much enlarged support. There are few who challenge that the R & D effort is essential to solution of some of the more pressing problems of our society. The great social revolution of our times was begotten by the previous successes of the industrial, scientific, and agricultural revolutions. The condition of our nation at the turn of the next century will be determined by the research accomplishments of the few years which remain.

Note

This article is adapted from a statement presented at a symposium at the annual meeting of the National Research Council, Washington, 11 March 1967. In preparing this statement, the author has drawn heavily upon his experience as a member of the National Science Board, the President's Science Advisory Committee, and the National Advisory Council for Health Research Resources, but the views expressed are not necessarily those of these official bodies.

Educating for the Scientific Age

DON K. PRICE

> *"Today, the belief in the possibility of a clear separation between objective knowledge and the pursuit of knowledge has been destroyed by science itself. In the operation of science and in its ethics a change has taken place that makes it impossible to maintain the old ideal of the pursuit of knowledge for its own sake which my generation believed in."*
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> *—Max Born, My Life and My Views.*

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What must a responsible citizen know of science? Plato, of course, started this argument. The king, he said, must also be a philosopher if the state is to be well governed. He wanted government to be based on absolute truth, and hence in the hands of those best equipped to discover truth. The modern world would translate Plato's "philosopher" as "scientist," whether he liked it or not.

Huey Long complicated the problem for Americans by his dictum: "Every man a king." No one thought of him as a political theorist, but he summed up in this phrase the American populist thinking of the nineteenth century, which has by no means lost its force in this the twentieth century.

And so when we ask how to educate for the scientific age, we are tempted to compound our prescription of equal parts of Plato and Huey Long. If the king must be a philosopher-scientist, and every man a king, then every man must know science. We would like to hold on to the fundamental principle of political equality and yet make the maximum use of modern science. But this compound presents some difficulties—perhaps even some possibilities of political explosion.

The subject that has been assigned to me is by no means a simple one, but no one should let mere incompetence deter him from tackling it. Science has forced the general citizenry to face up to some problems that were once considered purely speculative, and suitable only for academic discussions.

Philosophers used to argue whether the good life consisted of the cultivation of the mind and spirit, or of material prosperity and the gratification of the senses. Now we vote in appropriations hearings on whether to give priority to basic research or to applied technology—to graduate fellowships or to moon shoots and agricultural extension.

Scholastics used to argue whether those who pursued the higher learning should govern their own communities while receiving public support or should be fully accountable to the magistrate, and whether the laws of conscience overrode the laws of kings and parliaments. Today budget examiners quarrel about overhead allowances on research contracts, and unruly students are disciplined not for dissolute living but for protesting against the connections of their professors with industrial and military power.

There are two ways, it seems to me, in which questions like these, by moving from the academic cloister into the committees of Congress, have greatly complicated our notions of what the responsible citizen should know about science.

The first is that science and scientists have come to be more than merely instrumental forces to help carry out purposes that have been predetermined by others—either by the traditional custodians of religious values, or by the will of the electorate. They have become a powerful influence in altering values and guiding the purposes of national policy. The second is that science can no longer stand apart in complete independence from the flux of political controversy, and thus appear as a clearly objective source of truth. For when research must be supported by government grants, science itself becomes a part of the political system.

And so we are now at a point where we need to rethink the fundamental relationship of knowledge to power, of science to politics, in our society. We can no longer put together Jeffersonian optimism about the liberating effect of science with Jacksonian optimism about the universal competence of the average citizen, and make the combination work in an era of relativity, existentialism, and the prophecies of a psychedelic paradise.

SCIENCE AND CITIZENSHIP

A generation ago a good many scientists were tempted, even in the United States, to think that the Marxists had developed the only systematic approach to a political theory that proposed to make full use of science, and incorporate it into the processes of government. The theoretical attraction of that system, such at it was, has been greatly reduced by observation of the way it has worked in practice. Yet I must admit that, by comparison with the Marxists, we in the United States have been naive and uncritical in our approach to this crucial problem of the relation of science to politics. At a time when natural scientists have been insisting on the importance of fundamental theory for the hard sciences, they still tend to be merely evangelistic in their views of the kind of scientific knowledge required in a political system.

If we are to decide what scientists should undertake to teach other responsible citizens about science, we should base it on a theory about the way in which science is related to citizenship or to the conduct of political affairs. We have had no such explicit theory, but several contradictory theories have been implicit in the way we have behaved.

One such theory is idealistic and optimistic in its view of both science and politics. (I use the word "idealistic" here in its popular, not its philosophical, sense.) This assumes that in a democracy the people decide issues; science may help us decide them wisely; therefore teach the people science. I do not sneer at this approach. It accords with my hopeful sentiments, and even in uncritical practice it is likely to do much more good than harm. But it has some serious limitations. The number of issues that might be selected is tremendous if not infinite, the range of sciences involved in them is far beyond the comprehension of even the most gifted scientists, and any selection among them is likely to be guided more by emotion than rational analysis. Moreover, the extent to which decisions on the issues depend on various types of science, or on problems for which science is inconclusive or irrelevant, is often debatable. Finally, even if a political theory is to be based on an optimistic view of human abilities, it has to acknowledge limits on the extent to which the average voter may be expected to understand science.

If one is idealistic about science but cynical about politics, two main approaches seem possible.

The first concludes that since science is now at the mercy of politics the average voter must be wooed by teaching him something about science, preferably awe-inspiring in nature. This grudging conclusion is in much the same mood as that of the great Tory statesman who, after Parliament had unwisely given everyone the right to vote, came out in favor of more popular education, saying: "We must educate our masters." Today, any scientist who is interested solely in the prosperity and autonomy of scientific institutions is tempted to follow a similar strategy. The more television shows that portray the miracles of research and that depict science as the key to health and wealth at home and the first landing on the moon, the stronger the political influence of the scientific community with the appropriations committees.

The second approach of those who are cynical about politics is what one might call scientific fundamentalism, which like other branches of fundamentalism is having some resurgence these days. This is the approach that sees that massive support from public sources is likely to come only for programs of technology, not fundamental science. The alliance of technology and power politics, from this point of view, seems so dangerous that scientists should renounce wealth in order to maintain purity.

This approach avoids the uncritical optimism of the theory that assumes that scientists and other citizens should ideally know the same things, and the impractical cynicism of the theory that assumes scientists should assume the inferiority of other citizens and manipulate their ideas. Yet I think it is inadequate for the future, since it does not acknowledge the extent to which science is now inextricably involved in politics. It would work only if we could assume a political system guided by the establishment of some traditional values, or a system in which scientists themselves would be given the power to define new values for society. Neither idea seems very feasible for a country whose politics is controlled by the jostling competition of pressure groups under the continuous impulse of a dynamic technology.

A SOUNDER APPROACH

If we are going to try to discover a sounder theory, perhaps we should start by considering the ways in which science has been related to other fields of knowledge and to the formulation of basic values, not only in the larger arena of government and politics but in the smaller arena where the scientist as such feels more at home—the university. I am intrigued to observe some parallels between the ways science has been related to the reform of university curriculums, and to the constitutional and administrative reform of the government of the United States, over the past generation or two.

In both fields the critics started by making similar diagnoses of the ailments of contemporary American civilization, prescribed somewhat similar remedies, and ran into similar difficulties.

First, the diagnosis. Critics were dissatisfied with a world in which great scientific knowledge and technological power were under no control by a responsible poltical system, informed by a consensus of civilized and humane values. Some, of course, blamed this state of affairs in part on science, which they held responsible over the past couple of centuries for the destruction of a belief in religious and ethical values. Others tended more to emphasize the degradation of both the intellectual and political worlds by the dogmas of mass democracy.

One could trace this degradation in the institutional patterns of both the universities and government: on the university side, the free elective system of courses for the students, and freedom of the universities—with no ministry of education to set uniform standards—to appoint any number of professors, who could then teach any subjects that struck their fancy. In government, no disciplined leadership in political parties, no responsibility of the executive to the legislature, no career system for general administrators.

To the sensitive intellectual aristocrat, the connection of all this chaos with the growth of science and technology seemed clear. In the universities, the physical and biological sciences had acquired an intellectual and institutional status superior to the humanities and social sciences, which undertook to imitate their quantitative methodology. Financial support went to the applied sciences, and to the fields that provided for their practical application—the engineering and medical schools, and the great variety of agricultural and technological colleges. Similarly in government, the top ranks of the civil service that were freed from corrupt patronage went to the scientific and technical and professional personnel, not to the administrator with an education in history or the humanities. And the technical bureaus, under technical leadership, maintained a high degree of independence of responsible political leadership, with support from private pressure groups and specialized Congressional committees.

For education and for government as well, the prescription was implied by the diagnosis. In some universities, the general education movement of two decades ago sought to give students a grounding in a unified educational experience designed to strengthen their appreciation, as well as their knowledge, of the intellectual heritage of Western civilization. This approach was extended even to the sciences: those who were not to become specialized practitioners of the hard sciences were to study the earlier history of the sciences, the great scientific writers of the past, and the development of the major stages in the philosophy of science.

AVENUES OF REFORM

Scholars on the lookout for reforms were quicker, of course, to see the beam in the eye of government than the mote in the academic eye. Well before the general education movement took form, the typical political scientist was persuaded—following Woodrow Wilson—that party discipline and responsibility had to be established by some constitutional reform so as to bring our disorderly Congress under the guidance of some coherent ideas. Even more clearly, the civil service should be reformed so that general administrators, educated in liberal and humane subjects, should translate political policies into integrated programs in which technical specialities would be subordinated to the general values and purposes of society.

In both these movements, the point at which their shortcoming became most apparent was where the natural sciences touched on human value systems.

If I may take the experience of Harvard University as an example, the weakest point in the original general education program, and the one soonest modified, was the way in which it proposed to teach science to the student who was not to become a professional scientist. The original idea was that general education courses should deal in some sense with classic works and historic themes. But most of the natural scientists soon gave up the idea entirely, and insisted that general education should (without giving up its original humane purpose) instill a more concentrated knowledge of the fundamental principles of some particular science, and some idea of modern scientific methods. And even in the humanities and the social sciences, courses of a primarily analytic nature have come to be included in the program.

Similarly, American administrative reformers gave up as their goal an administrative class of the Civil Service based on an education in the humanities; instead they see that general administration, in many fields, must usually be built on a foundation of scientific or professional competence. I am tempted to draw three tentative morals from this observation of the ways in which we are learning to relate science to our intellectual and political value systems.

First, we cannot think of a value system that is apart from our science, and which we can either teach potential scientists independently at the outset of their training, or set up as the governing political standard apart from and superior to science. The determination of basic values, like the determination of policy, is not separable from the continuous processes of discovering knowledge and applying it to practical affairs.

Second, to connect abstract scientific knowledge with concrete value or policy judgments is no one-step affair. We have seen too many of these simple but unwarranted translations of scientific knowledge into moral, political, or theological principles. Thus relativity was taken to disprove the validity of moral standards, and indeterminacy was grasped as a reassurance by those who saw their faith in providence upset by rigorous determinism. If a responsible citizen wishes to know something about science, he must study not only its experimental data, but also the processes by which those data are related to value judgments and political action.

Third, these processes are affected not only by the formal published data of science, but by the inner disciplines of the scientific community. Just as we know very little about government if we only read the Constitution and statutes without looking at the systems of incentives and morale that hold the administration together, so we know little about the influence of science on government if we study only what scientists publish. What professors teach students by their example about the ethics and obligations of the practice of science is the most important lesson in the relation of science to values; what the students learn tacitly by the internal government of their own faculty departments and their own laboratories is more important for their relation to politics than the formal laws of the scientific texts.

NEITHER PLATO NOR HUEY LONG

These tentative observations lead me to believe that we must give up relying on either Plato or Huey Long as guides to what the responsible citizen must know of science, or to our theory of the relationship of science to politics. We must not assume that science, in its influence on policy, will simply operate under the control of traditional values, for science is going to have a hand in continuously shaping those values, or at the very least in shaping our thoughts about transcendental values. On the other hand, we must not assume that policy is made equally by all citizens, any more than than science is understood by all citizens; the key to the political problem must be in the relationships within society of men and women with different functions and interests, even though they have fundamentally equal status in the eyes of God and the jurists.

Neither the Platonic ideal of government by absolute truth, nor the populist distrust of authority and of intellectual discrimination, can serve as the theoretical basis for reconciling modern science with free and responsible government. Such a reconciliation need not wait, I hope, on the development of anything worth calling a fundamental theory, as scientists use that term, nor shall I attempt to provide such a theory. But it would get rid of the inconsistent and irrelevant models to which we try to make our system conform, and lay the groundwork for a more explicit theory, if scientists could tell other responsible citizens two types of things: first, how science is in theory related to other types of knowledge and to humane values; second, how in practice the scientific community and scientific institutions are related to the governmental system.

As for science as a branch of knowledge, the greatest obstacle to popular understanding is the ideal image of science which scientists have thought they needed to maintain for several centuries. When Bacon undertook to challenge Plato, he set up a picture of science which was useful as a defensive formula in the era when science was an academic poor relation, but is only a handicap now that science is the wealthy and predominant member of the house of intellect. This was the view that scientific progress depended on the inductive process, the accumulation of detailed experimental data from which general value-free truths could be derived by a coldly logical process.

This picture of science was partly true and exceedingly useful. It served the same purpose for science that the theory of the neutral civil service, with no role in the formulation of policy, served in the development of public administration. Both theories avoided challenging the dominant order, the custodians of established values; in the intellectual world, the philosophers and theologians; in the governmental world, the political leaders. And both theories were useful in that they disavowed any effort to establish doctrinaire theories or immutable principles that would let an elite hierarchy protect itself from outside challenge.

But both theories had the great disadvantage of obscuring the positive role of their subjects in the development of new ideas and new systems of values. Bacon's view that science was a method by which mediocre men could cumulatively develop understanding has, like Andrew Jackson's idea of basing a civil service on rotation in office, been a great handicap to the public understanding of the need for work of the highest quality.

If the lay citizen is to understand modern science, scientists must not be afraid to tell him three things that, on superficial consideration, seem to have elitist and anti-democratic implications.

First, science is not merely a matter of learning facts from nature, and then putting them together. It is also the creation of new ideas and concepts. The great scientists are those who produce new hypotheses, and by doing so revolutionize the way men understand nature.

Second, the most extraordinary advances are made by a few extraordinary men. Science as a whole requires a great deal of ordinary work, down to grubby technical routine. But the most creative part of the business comes from a tiny minority who must work on their own ideas in their own way.

Third, the way to unify the approach of the several sciences to human problems and concerns is not to learn a lot of miscellaneous facts in various fields of knowledge, but rather to learn—indeed to practice—some particular science in specialized depth, and then to grasp the connection between its fundamental abstractions and those that underlie the other specialized disciplines.

SCIENCE AND THE LAYMAN

It is customary for laymen, I know, to say that scientists have an air of arrogant superiority in talking with laymen about scientific matters. Surely some of them do, sometimes. But I am rather more impressed by the tendency of many of them to pretend that science is nothing very special—only systematic commonsense. Perhaps it is, but commonsense is such an uncommon item of commerce that it ought not to be undersold, even in the interest of accommodation to the democratic tradition.

It would be far better, it seems to me, to teach the responsible layman not that science is easy enough for him to understand—for that is not true, and he will soon know it—but that science is too difficult even for the scientists. The lay citizen will gain little by attempting to assimilate the factual knowledge accumulated by scientists. He will gain a great deal more if he tries to understand what some scientists mean when they say that their factual knowledge falls short of an understanding of reality, how they disagree and change their minds on the interpretation of the philosophical significance of science, and how they have come to have less confidence—here I think I understate the point—that any single system of science can solve any major problem of human values.

The present mood of disillusion and despair among many members of the intellectual community throughout the world may be based, in part, on the frustration of the exaggerated hopes that philosophers built on science. Science helped to break down the old system of theological ideas, most conclusively where those ideas attempted to deal with an interpretation of material phenomena. Those who were temperamentally or traditionally uncomfortable without a single clear source of authority then turned to science as the potential basis of complete and infallible truth. They got more encouragement in this hope a century ago than they do now. And the disillusion has been greatest where the hope was highest. The places where there is the least departure from nineteenth-century optimism seem to be those where science was never thought of in connection with philosophical theory, but only as a source of useful knowlege, and a means to health and wealth.

I am inclined to believe that even the layman is capable of understanding, by analogy and inference, that modern science does not oblige us to take either horn of the dilemma and to treat it as either (1) a mere servant of technology, with no relation to human interests or human values, or (2) a single philosophy and method that must be the basis of all our value judgments.

On the contrary, science consists of various types of intellectual activities. Some of them are matters of observing and measuring things and events, others of mathematical constructs and calculations. These are the domains of science within which there is the greatest chance of resolving differences of opinion, and most scientists like to stick within their boundaries.

But on the frontiers of knowledge, where new hypotheses are opening up new areas for exploration, there is continuous debate between those who choose different methodologies, and different guiding concepts—one might even say, different policies—for the advancement of science. In addition, there are the broader philosophic themes and issues that underlie the thinking of scientists, or that represent extensions from their purely scientific thought to the realm of personal philosophy. In the past, some scientists have been inclined either to dismiss such questions as metaphysical nonsense, in order to get on with what they can prove by observation and calculation. Others have been ashamed of the continuing difference of opinion within a field of knowledge where opinions should surely be subject to clear proof or disproof, and have had the normal human faith that the rest of the world must soon come around to their own opinions.

A CHANGE OF MOOD

All these uncertainties and controversies have produced a mood of deepening pessimism among some of the most thoughtful and philosophical scientists. And perhaps the disillusion and despair of our present generation is related to this change of mood. The intellectual progress of the world a century ago was being measured by the advancement of science, just as its material progress was being measured in industrial and technological terms. If we lose faith in the possibility of gaining certainty through science, where can we turn except toward the world of private consciousness, where the only standard is the gratification of the impulse of the moment?

Now the line of thought implied by this question, it seems to me, is nonsense. When we think of science in relation to our moral and political ideas, we seem to be charging science with two contradictory offenses. We reproach it for not always being certain, and especially for not giving us answers to the questions that trouble and divide mankind. And then we fear that it will destroy our freedom of responsible human choice by being certain about everything.

Once the cake of custom has been cracked, once traditional political authority has been shattered by the same attitudes of critical inquiry and individual responsibility that fostered the flowering of science, the foundation of political responsibility must be laid, in some sense, on our popular conception of a theory of knowledge. The traditional theory was one of revelation, interpreted by an ecclesiastical establishment and a legal profession. When that theory became untenable, one school of thought undertook to build its approach to politics on the possibility of scientific certainty, the vision of a single systematic science, interpreted and established by an authoritative elite, covering all branches of knowledge from physics to politics. And this the Marxist vision was adopted most enthusiastically where the transition was most abrupt from one version of infallibility to the other.

The liberal West, on the other hand, was more than a little confused on this issue. As a practical matter, scientists found it more rewarding to avoid metaphysical and political pretensions and to justify their status and support by the contribution they might make to material prosperity. But in theory they were tempted by the vision of LaPlace, who held to the hypothesis that if one could determine the current status of all atoms one could predict the future to eternity, and by the predictions of Comte, who expected sociology to be reducible to the same quantitative methods as physics and then to lay the basis for personal and political value judgments. Consequently, scientists managed to give the layman the impression that they were materialistic and lacking in concern for humane values, and yet at the same time possessed of a dangerous potentiality for controlling society's most fundamental decisions.

If scientists can now communicate to the laymen what most of them really believe about such issues—that is, the principles they act on rather than the ones they repeat by rote—they would turn upside down what the responsible citizen now tends to think about the political significance of science. They would lead him to see that the driving force of great science is not the accumulation of random facts in the hope of making material profit, but the search of a disciplined mind for the underlying principles by which man can understand some aspect of the universe. They would make it plain to him that the general problems with which he—the citizen—is most concerned are not going to be solved by any one of these several approaches to knowledge. Indeed, the more important the question is to the citizen the less likely it is that any one science can solve it, the more necessary it is that many sciences be brought to bear on its solution, and the more the immediate action on it must be guided by a type of responsible judgment that cannot be determined by scientific procedures. In short, the citizen might learn that contemporary science, by its fundamental nature, is no more inclined to lead us toward the science fiction dictatorships of the future than to the autocracies of the traditional past.

NEITHER KING NOR SCIENTIST

From these elementary and obvious points we can move on to the applied problem, namely, what can scientists teach the responsible citizen about the institutional status of science in society. And here, if Plato is not to be our guide, neither is Huey Long.

Not every man is going to be a king, and not every man a scientist. Within a system of responsible democratic government, power is not simply divided up equally among the sovereign people. Subject to ultimate checks, it is distributed on the basis of various types of competence. And as between the two types of citizens —the scientists and the laymen—the most important among the checks and balances in our social system is the tacit constitutional agreement that gives science (along with other forms of scholarly inquiry and teaching) the right of complete freedom to deal with knowledge, and a good bit of material support as well.

The basis on which the scientific community governs itself is not prescribed by law; it is substantially the same in public institutions like state universities as in the nominally private universities, whose responsibility to the public is recognized and enforced through a different set of institutional forms. Scientists learn the internal political system of their own special community not by any formal system of instruction but tacitly, through a kind of apprenticeship method. It is a subtle and complex sub-government, quite different in principle from the general public government.

In some respects, the sub-government of the scientific community seems to the layman shockingly undemocratic. Its system of incentives includes a system of honors and degrees and titles that rivals the British peerage or the nobility of the Holy Roman Empire. You cannot settle its big problems by majority vote. Nor can you settle them either by compromise in a committee, or by the exercise of executive power. And the accepted version of truth at any given time is an intolerant authority; those who do not accept the major concepts of the discipline cannot work within it.

It will not hurt the general public to be told that the advancement of science requires the existence of institutions that are governed on non-political terms, even when they need material support from political authority. Science is now too big and important a force in society to be supported without some understanding of its basic nature. The recurring series of bureaucratic and legislative squabbles over such issues as overhead payments, time and effort reporting, and the terms and conditions of grants-in-aid suggest that science has everything to gain and little to lose if it tries to tell the responsible citizen frankly what it needs for its basic care and feeding. And among those basic needs are freedom from the types of detailed administrative and legislative controls that may be suitable for the purchase of hardware or the delivery of the mails.

The second point on which the scientific community needs to come clean with the lay citizen is on the relation of applied science to applied values—in the world of practical politics, the relation of science to policy decisions. Since the layman may ask how the power of science may be kept in responsible bounds, the most important things he needs to know about science are the nature of its inherent incentive system, and its relation with the scientific professions.

The most important point, it seems to me, is that science if left to itself is not inclined to organize into a monopoly. There are countries where scientific institutions are organized into one big union, but they are the countries where political power organizes and controls the institutions of science, and permits no free initiative in such matters. In countries that encourage free associations, scientific societies follow the tendency of science itself to divide and subdivide into specialized interests.

Moreover, political and economic power comes not from the possession of fundamental knowledge, but its application, and the leaders of the scientific community are not those who make their reputations in the applied sciences. The high honors of the scientific world, including the tenure professorships in the primary scientific disciplines, go to the men who deal in the fundamental concepts and abstract theories of basic science. But the power and influence in practical affairs go to the professions that synthesize various specialized disciplines and apply them to concrete problems.

A generation or two ago there was still apparently great danger that the lay citizen and the politician would from sheer ignorance not appreciate the potential contribution of science to the solution of policy problems. There is still plenty of ignorance about and lack of appreciation of the power of science. But at more influential levels of opinion, it seems to me, the greater danger (especially for the biological, and even more for the social, sciences) is now the opposite one: that the public and politicians will not understand why science does not have the answers to public issues as the politician would state them, and why scientific institutions need to be supported without too much regard for short-term practical results.

The politicians cannot be expected to understand this point, of course, until scientists understand the complementary point—namely, that responsible political power should go to men not because they have the most competence in specific scientific fields or the highest abstract intelligence, but to men (even if they be scientists) because they have demonstrated qualities of administrative or political responsibility that cannot be measured in terms of knowledge alone.

The relationship between knowledge and power—between the institutions that foster science and those that govern public affairs—is the crucial problem in the maintenance of a free and responsible constitutional system.

If we start by looking on the sciences as humane studies, and their disciplines as a part of the intellectual equipment of the man who wishes to understand modern civilization, we have made the first step toward seeing the relation of the sciences to man as a part of organized society, the political system. There are many ways to take that first step, to approach science in a more humane and philosophical manner especially for men who are not undertaking to become professional scientists, and I suspect that no one will ever discover the one best way. This first step—which should be open to a considerable proportion of our citizens—should include an effort to learn some substantive science, in as much depth as one's capacity and circumstances permit.

It is important, I think, for scientists interested in the humane significance of their disciplines to recognize that there is a second step to take—the step toward recognizing the political significance of science. The natural sciences and the social sciences as well have been preoccupied with the development of specific skills, and specific instrumental techniques, to enhance man's understanding and control of the universe about him. But that universe includes his fellow men, and *they* are beginning to join with us in asking some of the old fundamental questions of politics—questions relating to power and justice and freedom—about the purposes and the limits of the new power that science and scientific institutions may exercise in our constitutional system.

Science and technology and industry have destroyed the American political dream of a nation in which all men shared, on relatively equal terms, in the knowledge and power by which they were governed. We now live in a society that is too complex and too dependent on special knowledge and special skills to guarantee its freedom by going back to that Eden. The scientist who first comprehends the connections between his professional specialty and a broader humane learning is in the best position to help first his fellow scientists, and next his fellow citizens, understand the problems they must solve in making science a force for freedom in a dangerous and troubled world.

Federal Funding: Categorical vs. Bloc Grants

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My purpose today is to bring into focus the Federal-State relationships affecting the way HEW programs are managed. We have developed many ties between the Federal Government and the States and local governments in connection with Federally sponsored activities, but no one has really articulated the philosophy of this relationship. It has been evolving since the mid-1800's, and, since the Depression days of the 1930's, it has grown somewhat like Topsy. Its recent growth has given rise to concern as to what kind of partnership we have with the States and as to who is in charge.

Today I would like to discuss one concept of the Federal-State relationship. This concept is in no sense an Administration position, nor does it represent a decision on anyone's part that it is *the* proper relationship. Before any program reflecting this concept could be adopted, it would be subjected to a great many discussions —a great many abrasive encounters that would polish and hone it until it evolved into a line of policy that an Administration could support.

This presentation is intended merely to open a dialogue on the long-range possibilities for the Federal-State relationship. It will deal exclusively with categorical grants to States from HEW. In the interest of clarity, I have, for the most part, excluded from consideration the problems of public assistance. Public assistance is an open-end appropriation involving a Federal-State relationship quite different from that of the other HEW programs; also, it is so massive that it tends to impair our perspectives on the other programs.

The original plan for returning a portion of Federal revenues to the States without earmarking was developed by Walter Heller, chairman of the Council of Economic Advisers, working with Joseph A. Pechman, a consultant from the Brookings Institution. Their formulation sparked widespread discussion, and many alternative approaches to the present grant-in-aid system. The Heller-Pechman plan was developed as part of an effort to portray a full employment economy under peacetime conditions. This portrayal suggested that the present-day tax structure would in future produce large sums of money in excess of the expenditures projected for the Federal programs which were authorized at that time.

Heller and Pechman identified three alternative methods of using the surplus revenues: (1) to modify the tax structure so that the Federal Government would take a smaller portion of the money and permit States and localities to use the remainder for new programs, (2) to develop additional Federal categorical programs, or (3) to continue the present categorical programs and return the surplus money to the States to use at their discretion. The two economists endorsed the third alternative.

It seems strange to be talking about large surplus funds in a time of deficits like the present. Yet, rather than waiting to confront an issue of this kind under crisis conditions, we should deal with it when we have the leisure to talk it out and plan for it without the sense of urgency which often leads to precipitous and ill-conceived action.

The national concern with the categorical grants system has been expressed by the Governors of the States through their conference, by the Intergovernmental Relations Commission, and by the House and Senate Committees on Government Operations. All have evidenced concern over the increased complexity of Federal activities and with the possibilities of overlap in the uncontrolled mushrooming of categorical programs.

Federal officials have become similarly alarmed as they have become aware of how the interrelationship among programs can affect the administration of any one of them.

Why this growing concern? One reason is that the number of categorical programs has been growing rapidly. Another reason is that the programs have not been designed to insure mutual exclusiveness. On the one hand, we have programs designed to satisfy functional needs of health, education and welfare. On the other hand, we have programs that were set up to handle specific age groups, such as children and the aged. Some are geared to economic conditions—programs for the poor, for example—and others work on categories of people, such as the disabled. Still other programs were designed to deal with processes: the planning of Federal projects, their operation, the construction of facilities, and demonstration work. As these programs have grown, we have found that similar functions in different programs were being administered by different agencies. This is true within the Department of Health, Education, and Welfare and it is true also in other departments of the Federal Government.

Administrative complexities in the programs have become of increasing concern as the activities have grown more extensive and have begun to overlap. We have variations in matching requirements, in eligibility requirements, in regulations and procedures. We have varying reporting and accountability requirements, varying degrees of technical assistance, varying degrees of monitoring and supervision of programs. Finally, we have varying arrangements for identifying eligible grantees. The grantee may be the State, the locality, or a nonprofit agency. Within the State it may be the Governor, a designated department, or a designated agency. Within the locality it may be the city, the county, the school board, or some other entity. There is no consistency in the selection and identification of eligible grantees.

President Johnson, in a speech at the University of Michigan on May 22, 1964, propounded the idea of "Creative Federalism" to achieve the objectives of the Great Society. He defined Creative Federalism as a collaboration of the States, the Federal Government, and the communities to fulfill the aims and aspirations of our society.

A significant step toward Creative Federalism, and one which affected the programs of the Department of Health, Education, and Welfare, was the enactment of the Partnership for Health program. This was a major departure from the pattern of establishing minor and categorical programs and then continuing them for long periods of time. Instead, it established the idea of systematic re-evaluations of needs under a comprehensive plan.

Departing from the usual practice of specifying in the statute the portions of the resources which would be allocated to tuberculosis, venereal disease, chronic illness, and other limited categories of problems, the new program substituted categories of processes. It established a category for planning and called upon the States to develop both an organizational mechanism for planning and a comprehensive plan in the health field that would take into consideration the total needs of the State—those that would be carried out by the private sector, those reserved for the educational sector and those assigned to the public sector.

Secondly, the new program provided that a substantial part of the funds would go to the States as bloc grants to insure the operation of their comprehensive plans. This money, together with their own funds, would help the States and communities to implement their plans. The program also set aside special project funds to give the Federal Government new flexibility in spending—to demonstrate new ideas, for example, and to follow through on the results of research and experimentation with changes in the management of the health scene.

The President's message to Congress on better government suggested that the achievements of the Partnership for Health could be emulated in other social programs operated by the Federal Government through the States. About 94 percent of the programs of the Department of Health, Education, and Welfare are carried out by non-Federal entities, most of them state agencies. In Fiscal 1967, the Department of Health, Education, and Welfare administered 43 separate categorical programs of formula grants to States, involving $6.9 billion, as well as 15 separate categorical programs in the form of project grants carried out through State agencies, involving $300 million.

Broken down by functional area, 18 of the programs were in the health field, 26 in the educational field, eight in rehabilitation and six in social services. Looked at another way, five programs were related to planning, 33 to operations, nine to construction and 11 to special project grants.

An interesting fact about these programs is that 35 of the 58 were created in the last five years. Thus, in spite of the success of the Partnership for Health approach, with its limitations of categories, the number of categorical programs is escalating by leaps and bounds. It is not sufficient to say, "*Let's just take a look at each of the categorical programs and determine which ones can be eliminated and which ones can be merged.*" Rather, it is necessary to evolve a philosophy which will have an influence not only on the operation of existing programs but also on the choice of programs to be developed during the next few years. Unless we arrive at a preventive approach, we will never be able to eliminate and merge categories as rapidly as we develop new ones.

The purpose of my talk, therefore, is to establish a philosophy of shared Federal-State relations in program operations, and to develop a viable approach to carrying out such a philosophy. It is, further, an attempt to develop a phased implementation of the philosophy. We must realize that we will not be able to carry out a modification of the present relationship between the Federal Government and the States in one clean sweep. We will have to move gradually and to test, perfect, and improve the techniques as we proceed.

I would like to develop for you a conceptual model of such a philosophy and its implementation. There are a number of elements that should be built into such a model. The first is the need to expand state responsibility and options in carrying out Federal grant-in-aid programs. There must be less Federal direction and more authority for the State agency which can be more responsive to local needs. The second element should be an effort to minimize the number of categorical programs as much as possible in line with national objectives. We should not say arbitrarily that there can be only a certain number of categorical programs, but we must remember that a limited number of categorical programs is more likely than a large number to facilitate a partnership arrangement with expanded State responsibility.

Another factor, which was involved in the Partnership for Health program and which is worth emulating, is an emphasis on comprehensive planning and objective evaluation. It is not sufficient merely to turn responsibility for a program over to the States. It is not sufficient merely to outline the individual programs and services. It is important that these things be fitted into the broad context of a plan covering a long period of time; it is also vital that there be an organized system for evaluating the effectiveness of the work and determining how well the objectives of the plan are in fact being achieved.

An additional element to be considered is the distinction between funds provided for support and those provided to assist in the development of facilities. The nature of the budget process used in the United States for public service makes it next to impossible to trade off between the construction of facilities and the payment of personnel for day-to-day operations. Most States and localities use some form of capital budgeting and without a system which amortizes the capital investment in programs, it is impossible to make an effective trade-off between capital cost and operating cost. Thus it seems best, at least for the time being, to keep the two separate.

It should be recognized, I think, that there is a need for categories if only because they make it possible for the Federal Government to exercise a leverage directed toward achieving certain goals in areas of national concern. In putting out bloc funds for health, education, and welfare, the government cannot make its influence felt so effectively.

At the same time, there is a value in limiting the time element in dealing with categories of national concern. Let me give you an example. Several years ago we developed a program designed to revolutionize mental health care by moving away from large custodial institutions in remote population areas toward community mental health centers. If we had used the Partnership for Health approach and relied upon each state jurisdiction to work out such changes through its plan, it is unlikely that very much would have happened. But, by creating a category that provided first for the construction of the facilities and then for their staffing and operation, we are in fact bringing about a revolution in the handling of mental health.

The question is whether we must provide a permanent category of support and assistance or whether we can set a time period for accomplishing our objective and provide that the category will exist for that length of time only. At the end of the specified time period, we could abolish the individual category and return to support from the broad bloc grant. We would no longer need to be concerned with the category, and there is little danger that it would fade out of the picture. The laws of physics contain a statement to the effect that things in motion tend to remain in motion, and everyone knows that bureaucracy is capable of fighting long and hard for the retention of a program once begun. The inertia associated with the modification of programs is in itself a safeguard against precipitate changes or deletions of programs, and an assurance that if change comes it will be part of a comprehensive plan.

Another element of consideration is the idea that in making the major changes described here, it becomes necessary to ease the transition whenever feasible. Once responsibility has been delegated to a state agency, we can help to assure that the responsibility is discharged well by providing the kind of expertise, the kind of knowledge, the kind of background that will assist the planners and decision makers at the State level to plan well.

The final element to be considered for the conceptual model is an emphasis on accountability to an informed public. One trouble with the present grant-in-aid structure is that even the well-informed citizen finds it difficult to know where to lay the credit or the blame for the health, education, and welfare services being provided in his community. Is it local elected public officials, State executive officials, State legislative officials, the Federal Executive Branch, or the Federal Legislative Branch that is responsible? We have so diffused the interrelationships of the Federal Government, the State government, and the local government that an informed public cannot detect a sense of accountability and does not know where to exercise its leverage to bring about change.

A conceptual model has been developed to deal with these elements of consideration. To meet the long-term objective for a limited number of categories, it is suggested that four major functional categories be established to carry out HEW responsibilities: health, education, rehabilitation, and social services. There are some who would favor a larger number of major categories, and some who would favor a smaller number. The four specified here commend themselves through certain common attributes. They are broad, well-recognized, and frequently organized separately. They tend to serve special interest groups, and they tend to coincide with Congressional organization of legislative committees. I might add that they do not do any of these things all the time.

It is proposed that we subdivide each of these major functional areas to provide for separate funding for planning and evaluation, operations, construction of facilities, short-term categories, and special projects. The planning and evaluation funds would not be made available solely to launch new programs. They would be issued on a continuous basis in recognition that thoughtful and comprehensive planning must be carried on at all times if we are to have effective programs, and that the evaluation of how well we have done in the past is an integral process in perfecting our ability to plan for the future.

Funds for operations would be provided as formula support grants available to the States for programs involving services. This would provide for basic support funds as distinguished from the more intermittent allocations for stimulation and innovation. It would constitute the use of the Federal taxing power to share the burden of health, education, and welfare services with the States and localities.

Construction funds would be provided on a project-by-project basis. They would provide, or help to provide, needed facilities to carry out health, education, and welfare activities.

Short-term categories would provide funds for stimulation; they would exercise leverage for new or expanded efforts to meet national objectives, as in the illustration previously cited in the area of mental health.

Funds for special projects differ from funds for categories only in the sense that they are oriented to a project-by-project approach, the projects being designed to undertake research, to demonstrate how the knowledge gained can be disseminated, and to encourage the adoption of new and improved services. The project grants can also be used for dealing with regional problems and with problems of limited national concern that do not merit the creation of a separate category.

The conceptual model assumes that no matching would be required for planning. Planning is so important to the conduct of the total effort that the readiness of a State or locality to supply funds for it should not decide the issue.

With respect to operations, the concept of matching loses its significance for such large functional areas as health and education. They are too extensive and the State and local involvement is too great. It is suggested, therefore, that a maintenance-of-effort criterion be employed so that Federal funds will be used to augment and expand services rather than to replace State and local funds.

It is proposed that construction be on a matching basis, and that local and private money be eligible for matching in addition to State funds.

Short-term categories, too, should provide for matching, and consideration might be given to increasing the amount contributed by the State during the life span of the category as a means of stimulating new services and then of melding them into the operation or support program.

Special projects, as is true today, would not require matching, but they would be restricted to a limited time period to prevent them from becoming a method of providing services.

Under this new approach to Federal-State relations, the Federal role would be importantly modified. The Federal Government's review of the State plan would be primarily for conformance with minimum requirements of law and regulation, and it would be primarily procedural rather than substantive. There would be no pressure to substitute the judgment of Federal officials for that of State and local officials.

At the same time, accountability to an informed public would be stressed, and so would the importance of a thoughtful review of the state planning. To this end, it has been suggested that we create regional panels of experts and knowledgeable public leaders who will evaluate, comment on, and make recommendations for improvement of each State's plan. Their conclusions would be well publicized, but their role would be advisory and their recommendations would not bind State officials. For example, the conclusions might point out to an informed public that a given State's plan provided significantly less activity for the child of preschool age than did the plans of the other States in the region. They might list some of the research findings that indicate the value of providing educational funds for the preschool child. This information might be persuasive to the State. The fact that the conclusions are highly publicized might exert pressure on the State to modify and improve the plan. The Federal Government, for its part, would be called upon to provide a greater degree of expert assistance than it now does. Its agencies, emphasizing persuasion rather than authority, would be expected to identify effective programs and provide model program analysis and evaluation.

Under this system, operational grants would go out on a formula basis and would be monitored for conformance to the State's plan. Construction grants, as at present, would be subject to project-by-project approval and would be required to meet minimum Federal standards. However, the State plan and the desires of the State would be considered in each such approval process.

The categorical grants would be monitored for conformance to the State plan and to minimum Federal requirements.

Special projects would be evaluated on a basis of objective and relative merit. The State's recommendations would be obtained on each project and approval action would deviate from the State's recommendations only in the most unusual cases.

We must recognize that a plan like this could not be put into effect all at once. It has been suggested that the problem might be approached by trying first of all to apply the conceptual model to new legislation as it is developed. Secondly, as legislation for existing programs expires, the programs could be modified to conform to the theory of this model in the extension legislation. The third phase would be to review on-going programs which do not come up for renewal within a reasonable period of time.

The model outlined here is meant as a first step toward a conceptual plan that could be applied to a large portion of the programs of the Federal government. Each program would have to be evaluated in terms of the model and would be expected to depart from it only where there was good and sufficient reason. On the other hand, the plan should not be so arranged that it is slavishly followed even in instances where the Federal program and the national objectives could better be furthered by a deviation from the concept.

There are some critics who say that the States are not ready to assume additional responsibility. They believe that the States do not have sufficient professional personnel, that their pay systems are not high enough, and that therefore, they must be guided and controlled by the Federal Government in the conduct of their programs. There is another side to this coin. Unless we begin to place responsibility and accountability in the hands of the States, we may find ourselves permanently in the position of saying that they are not ready for additional responsibility. The purpose of this proposal is to challenge the States and to give them a greater degree of responsibility than present programs do, and then to provide such aid and assistance as they indicate a need for. We have learned a great deal about how to go about this from the Partnership for Health program. We should capitalize on this experience as rapidly as feasible.

Support of Scientific Research and Education in Our Universities

F. A. Long

No doubt at any moment in time there are people who feel that that particular moment is critical. I say this in apology because I do feel that now is a critical time for the support of science. It seems to me that we are approaching a major decision point on how we will support science in the United States, and specifically on how we will support scientific research and education in universities. If the nation is to reach this decision wisely, it surely needs the most thoughtful inputs possible from the people most involved—the scientific teachers and research scholars. It seems to me therefore of great importance that university scientists think through the problem as clearly as we can, and that, when we have some sense of vision and need, we present our conclusions with vigor and persuasiveness. What I wish to do is outline some aspects of the problem, give some tentative suggestions of things for us to do, and, in general, attempt to initiate what I think is a most necessary and important discussion.

I thought of saying, but hesitated to say, that we had reached the end of an era. On the other hand, I have no such hesitancy in saying that some 20 years ago the United States, and especially its federal government, did embark on what has been a new era in the support of universities and in the relationships between universities and the federal government. I speak, of course, of the decision to support basic research and graduate training in universities by utilizing funds from agencies of the federal government.

It is not characteristic of the United States to make its major decisions in one swoop. Rather, we are inclined to embark on a new line of effort or a new policy by making numerous smaller decisions, all of which then add up to a grand and important total. I think this is a good description of what has happened in the relationships between the universities and the federal government. In a relatively brief period between, roughly, 1946 and the early 1950's we made a set of decisions of major importance—or, more correctly, we put in motion a set of actions which have become translated into major decisions. Let me try to put down what I think were the key things that were done during these important years.

1) We reached a national decision that there should be federal support of higher education, especially at the level of graduate training and research.

2) We decided that the universities would have a central role for the nation in the conduct of basic research in science and engineering.

3) We decided that support for higher education and basic research at universities would be accomplished through a multiplicity of federal agencies, including mission-oriented agencies, such as the Department of Defense and the National Institutes of Health, and agencies more directly charged with support of education and basic research, such as the National Science Foundation and the Office of Education.

These decisions did not come into being fullblown, but the results have been as important to the country as if

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they had. Certainly federal support of higher education is with us in a major way, and no one believes for a moment that the situation will markedly change. More precisely, no one sees the future in any other way than as involving increasing federal support for higher education. Similarly, most of us believe that the universities will continue to have a dominant role in basic research. The third element of our broad policy—that support will come through a multiplicity of agencies—is not so certain. There continues to be serious talk of an umbrella-like Department of Science. Personally, I am convinced that multiple-agency support will continue.

A historian may quarrel with my analysis of the U.S. decision-making procedures. He would not quarrel with the visible consequences. As we all know, the past 20 years have witnessed a buildup of major proportions in federal support to the universities. Very large amounts of federal funds have been granted under the rubric "research," particularly for efforts in the natural sciences, mathematics, and engineering. Support for the social sciences has been far from negligible, and, very recently, small amounts of support for the arts and humanities have come from the new foundation established for this purpose, but the principal federal research support to universities has been in science and engineering.

During this same period, fellowship support for graduate training by the federal government has also built up rapidly. Some of this has come from science-oriented agencies like NSF and NIH. But other and broader fellowship programs have been started in the Office of Education, notably the National Defense Education Act fellowship program. Paralleling these teaching and research funds has been major support to the universities for new construction—again, particularly for facilities for graduate study and research. And, finally, there has been substantial support for special education programs.

It is often charged that this federal support is unbalanced in its strong emphasis on science and engineering, and this charge has considerable justification. On the other hand, one must give the federal government very great credit indeed for two things. One is the very rapid rate at which support to the universities was increased. The second is the enlightened and flexible characteristics of the programs that were developed.

A New Situation

With all of this as background, the exceedingly important fact which we now face is that the growth of federal support for teaching and research in the universities has halted and the total support has perhaps even started to recede. This fact has been sharply dramatized by the necessity for NSF to put ceilings on university expenditures for the current year, ceilings which effectively lead to cuts of 20 to 30 percent in planned expenditures in the universities. Smaller but nevertheless real cuts have been made by NIH and other important support agencies. Clearly, we face a new situation. It is a situation which is doubly ominous—ominous in its immediate effects and in its longer-range implications. This is a matter of particular concern when put in juxtaposition with the fact that both total enrollment and graduate enrollment in universities continue to go upward. Even a constant level of support from the federal agencies will thus lead to diminishing support per student involved.

That these changes have been accompanied by increased signs of an overall public disaffection with science and science education is also ominous. To say that there is an anti-intellectual tendency in Washington and perhaps also in the country may be too strong, but at the least there is a generalized doubt, on the part of the public, that science is useful, and concern as to whether science merits the comparatively high degree of support that it has had in the recent past. Some of these negative analyses are reinforced by a parallel disaffection with the conduct of our universities. This has many facets, including reaction to student rebellions and reaction to anti-Vietnam demonstrations of one sort or another. But even thoughtful people are increasingly concerned about the relevance of many of the universities' activities to the nation's problems, particularly to such critical problems as preservation of our environment and racial justice.

A dedicated university scientist and teacher is particularly startled to find evidences of general disaffection just at a time when he believes things are going exceedingly well. In many ways he is right in this belief. By any reasonable standards, scientific progress in the United States is in excellent shape. To say that we lead the world in science and mathematics is a truism. It is also true that in our laboratories, and specifically in the universities, we have good, generally modern, facilities for research. And finally, on the question of adequacy of support for our graduate students, we can honestly say that considerable, if not always outstanding, support is available to almost all of them.

But at the same time that the university scientist makes this somewhat complacent analysis he must hasten to admit that the universities, and specifically the science programs within the universities, have a good many problems, some of them exceedingly serious. Within the traditional science fields there continue to be enormous pressures toward expansion. The total student body in the universities grows, and the number of undergraduate and graduate students in the standard fields of science continues to increase. New teaching and research facilities are needed for these students. Research is expensive, graduate training is expensive, and both clearly are going to remain expensive. Furthermore, there are steady pressures for setting up new programs in science and engineering. Thus, universities suddenly find that an important field of study called computer science is in their midst and needs support. New interdisciplinary efforts—for example, between biology and the physical sciences—are growing and also need support.

It is this strong sense of continued pressures which leads to the feelings of beleaguerment and dismay which so many of us share. At the individual-university level we know full well that we cannot stand still. If we do not build new programs and expand the best of our old ones, we are certain to regress. At the national level, this same feeling exists with respect to total research effort. We live in an age of technology, and in every direction the need for more and better science and more and better technology is upon us. If we are to solve our problems and avoid creating new ones, we must continue to produce research, and it had better be good research. Given this rather grim overall picture, we are forced to ask, What can we ourselves do to help obtain the support we all know our programs need?

Suggestions

A first and important answer is that there is no single solution which, if successful, will solve all our problems. The problems are a complex mixture of internal university problems and problems of external relations, problems of preserving old programs and placing adequate emphasis on new ones. Because of this complexity we can be sure that there is no grand answer to our dilemma; there are only many partial answers. Each of us will have his own list of things to be done. I shall name several which seem important to me.

1) In our analyses and discussions we in the universities must put first emphasis on the university as an *educational institution*. Correspondingly, we must emphasize the kinds of support that the educational programs of the university need. In my view, a number of items have conspired to lead to more emphasis on research in universities, and more visibility of the research efforts, than the facts have ever warranted. Thus, the accident that much of our support comes from mission-oriented agencies, which necessarily place little or no explicit emphasis on the educational parts of the programs, has surely been a major factor. So, also, has been the desire of support groups for explicit answers and clear indications of research progress. Education, unfortunately, lends itself neither to easy analysis nor to spectacular measures of new progress. About all we can measure is the number of students we turn out, with little or no possibility of analyzing the depth of their training or the relevance of that training to the world they go into.

But the fact remains that universities are, first and foremost, educational institutions, and we must increasingly stress the fact that a major fraction of the support we ask for and need is for educational programs, most notably for the programs of graduate training. One consequence of this, I am convinced, is that we must increasingly press the support agencies having responsibility for education, such as the Office of Education and the National Science Foundation, to recognize the need to support universities on the basis of their education efforts. It is, I think, a sign of the times, and a very good sign, that a recent bill introduced by Congressman Fraser of Minnesota is entitled the Graduate Education Act of 1969, and calls for support to universities according to the number of Ph.D. degrees they have awarded in the past 3 years. Of equal interest is the fact that Congressman Fraser was assisted by five University of Minnesota professors in drafting his bill.

A related problem to which we in universities must give more thought is that of education at the postdoctoral level. We are deeply involved in this, but we have not yet developed the educational justification for it to anything like the depth to which we have developed justification for graduate training. If we wish this sort of education to continue as a major part of our work, we must be clear in our minds, and persuasive with others, as to what it is and where it fits into an overall university program.

2) We must stress and document the synergistic aspects of the linkage between teaching and research which so notably characterize the U.S. university. University people are themselves strongly committed to the belief that teaching and research are mutually helpful. At the same time, I do not think we have made our case to the degree we can and should. Furthermore, we must make our case at at least two, and perhaps three, levels. We must give clear and persuasive answers to those who ask why participation in research is considered the best kind of graduate training. Why, specifically, is a research apprenticeship the best means of training a student at this stage in his career? We should make the same sort of analysis for postdoctoral education.

I think, however, that we must make, with equal force, a case for research as an increasingly useful component of undergraduate training, and, along with this, must explain why the conduct of research makes professors better, more persuasive teachers.

3) We must be more explicit about the importance of basic research to our nation's progress. All of us have talked about the importance of basic research to technology. Unfortunately, we are all to some degree inclined to give the illustration of Michael Faraday and stop there. To put it bluntly, the whole basic research establishment is vulnerable to the cynical but wholly understandable question, "What have you done for me lately?" I think we must try to answer this question. We must tell why the basic research which we are now doing is needed, and what social benefits it relates to. We must do our best to forecast the trends of technology and the kinds of basic research that are broadly relevant to them. Since scientists are not, in my opinion, very good forecasters, our answers may not satisfy even ourselves, but still we must try.

As for the question "To whom should we communicate?" the reply is, "To everybody." We must tell our story in a way that the general public understands and appreciates. Much more specifically, we must focus our efforts on those groups that have been charged to concern themselves with the progress of education and of science. This especially means the legislative bodies at the federal and state levels. The old injunction "Tell it to your congressman" is precisely applicable.

In stressing the urgent importance of presenting the university needs and accomplishments, I am not simply saying that we in universities must become an all-out political pressure group. Perhaps we shall turn out to be partly that, whether we like it or not, but surely the first responsibility we must accept is that of telling our story thoughtfully and, to the extent possible, objectively. We *know* that science is important and that science education is essential. This is the case we must develop.

4) We must do everything possible to make our university programs of teaching and research as effective and as efficient as possible. I still recall the sharp comments an industrialist once made at a large meeting of the President's Science Advisory Committee in discussing costs of graduate training and research in universities. Noting that the costs per student trained had been rising steadily for the past 30 years, he said, "Universities are the only group that I know of in the whole United States economy where the costs per unit operation have been steadily rising. If you were a component of my industry I would probably call in your management and ask for greater efficiency, and if I didn't see some signs of it pretty rapidly I would fire them and hire a new crowd." This may sound overdrawn and silly, but it underlines a concern to which we must address ourselves.

To take a very large and specific problem, can we justify our exclusive emphasis on research apprenticeship as the path to advanced degrees? That such apprenticeship should be the path to our highest degree, the Ph.D., is, I think, something on which we all agree, but we know this is an expensive, time-consuming procedure. Are there alternatives which produce useful professionals but which are less costly overall? I doubt if we are giving adequate attention to this sort of efficiency.

To consider a related point, we must surely do everything possible in our universities to utilize our research equipment as efficiently, and to teach our students as effectively, as we can. I am uneasy about analyzing education from the standpoint of efficiency, in the conventional meaning of the term. But surely this conventional meaning does apply to many of our research activities, particularly to our use of expensive or scarce equipment and facilities.

It is also important that we search for collaborative procedures among our sister universities to try to hold costs down. Cornell, for example, is exploring the possibility of collaborative efforts to share library resources with a half-dozen nearby universities. As scientists and educators we know of other areas that are comparably expensive and equally open to collaboration—for example, further centralization of computer equipment. Needless to say, not every effort toward collaboration will succeed, and not every one of them will save money, but we probably must try them all.

5) Scientists and engineers in universities must search for ways whereby we can participate in the applied research programs which link to the great sociotechnologic problems that we all see on the horizon. Not all of us can usefully contribute to the solution of problems of urban redevelopment or of air and water pollution, but some of us can, and probably we all should seek for possibilities. Most of these problems are complex and difficult and require interdisciplinary efforts in which progress will depend only partially on applied science and perhaps even less on basic science. But to the degree that the problems are science-based and to the degree that we, as citizens, recognize their importance, we should search for the places where we can help.

In the search, we may have to address ourselves much more sharply to the overall effectiveness of the university programs in these fields of applied research and sociotechnologic improvement. I think our record of accomplishment in these areas is not very good, especially where interdisciplinary actions are involved. Since I strongly suspect that there will be increased national emphasis in these areas, the universities may need to analyze and perhaps modify their procedures. Thus it may be that we need a much closer coupling to the governmental and industrial laboratories of applied research that will probably be charged with the action programs.

6) Before I turn to my final suggestion let me recall the first time that C. K. Ingold, of University College, London, came to Cornell as a visiting lecturer. At a large reception for him the somewhat ebullient wife of Cornell's dean was pressing Ingold as to why he had been willing to come to Cornell. Carried away by her enthusiasm she asked, "What persuaded you to come here? Wasn't it that you were impressed by the possibility of passing on your knowledge to a new group of students in a new land? Didn't the thought of communication among nations and need for international friendship loom large in your decision to come to Cornell? Wasn't it of enormous importance that you could be of service to such a large and different body of students?" To all of this Ingold nodded, saying, "Yes, yes, of course. And then," he added, "there was the money."

And this is, of course, our situation. Whatever else we have in the way of public understanding of our programs, the universities need money to support our students and update our facilities. I am convinced that most of this money must continue to come from federal agencies, and this is why we must work vigorously with the relevant congressional committees to persuade them of the importance of the university programs. We must persuade the Office of Education to take additional responsibility for higher education and especially for graduate training. We must attempt to get something like the Fraser bill enacted, and we must see to it that the National Science Foundation is increasingly well supported.

However, we must face the fact that, as of now, federal funds have leveled off and will probably increase only slowly at best. Hence we must look for other sources of funds to support our university teaching and research programs. One such source is the state and local governments. They have traditionally supported education, and they should be sympathetic to the serious needs of the universities. They must be persuaded that what the universities are doing is important, and that the way the universities are doing it is sensible and efficient.

In many of our searches for support for new programs and for better ways to do old jobs, we can turn to the foundations. They have always been a source of support for universities and, with continued effort on our part, should continue to be.

Finally, those of us who are chemists return to a source of support which we have long enjoyed and which we have perhaps neglected in our recent love affair with the federal government. I mean, of course, the chemical industry. It is the great good fortune of chemists that they have had such close ties with an industry that has looked to universities for much of its basic research and has depended on universities for a continuing supply of trained professional manpower. There are many pluses on both sides for a closer linkage between university scientists and the chemical industry. Each side can contribute toward analyzing new needs and foreseeing new directions. Industry can tell universities more clearly and carefully what is in store for the students who will be coming to the chemical industry, and can thereby help in the training process. Universities, in turn, can broaden their teaching responsibilities and play, as I personally think they should, a larger role in the continued updating of the older professional people in the industrial establishment. Industry and the university establishment can be closer and more mutually supportive in the conduct of basic research. Among the many likely consequences of these firmer links is one which relates directly to our current discussion—namely, more funds to the universities for their teaching and research programs.

You may ask, Is it reasonable to expect greatly increased university support from industry? Perhaps the best answer I can give is to say that there appears to be one country in which very large-scale industrial support does occur, and that is West Germany. According to the 1965 National Academy of Sciences report on Chemistry, even in 1960 the level of West German support for basic research from the chemical industry was the equivalent of $17 million per year. This is roughly the amount which NSF has allotted to chemistry research in the current year—in other words, a very substantial contribution. Perhaps our motto should be: If the German scientists can do it, so can we!

Summary

Let me conclude by saying that adequate support of science research and education in universities is a serious problem and one which demands the most imaginative efforts of the university scientists. I have tried to suggest a few things to do. There are surely many others. Perhaps the proper concluding injunction is, time is wasting and we had better get cracking.

Basic research: its functions and its future

A symposium-in-print

Our symposium is based on addresses given at the last Atlantic City national ACS meeting. The symposium was sponsored by the ACS Committee on Chemistry and Public Affairs

Basic research in science is virtually synonymous with science itself. Our entire civilization is increasingly based on technology. Since these statements can be made without equivocation one may legitimately ask what is the problem? Why a discussion on the function and future of basic research?

The reason is that despite the benefits of science there are significant questions and concerns. Penicillin and polyamides are products of science, but so are nuclear weapons and napalm! People ask why basic research costs so much, must so much be done, must it dominate university programs?

Basic research has moved from the Joseph Priestley's working in their kitchens to great national laboratories such as Brookhaven with its 3500 employees and an annual budget of more than $50 million. Basic research has become institutionalized, instrumentalized, and expensive!

On the last point, it is terribly tempting to jump to the question: Is it worth it? But that is a trap that should be avoided because cost-effectiveness studies, to use current jargon, may make no sense at all. Cost effectiveness is a fine criterion for feeding poultry, but can it reasonably be applied to the expansion of the human mind?

AN INTELLECTUAL ENDEAVOR

ROBERT S. MORISON, *Cornell University*

The one thing that everyone seems to know about science is that it is an intellectual activity. Indeed, the requirement for brains may be overrated in comparison with the need for certain other character traits such as curiosity, patience, perseverance, and courage. To do *great* science one needs something in addition, which no one has ever been able to define very closely. That is "creative insight."

Just where intellect ends and creative insight begins, or whether creative insight is really different in quality or simply more extraordinary in degree than everyday intellectual activity, is difficult to say. People who seem to have creative insight in science say that it is more like the activity of poets than it is like a more conventional intellectual activity, such as chess playing or preparing a legal brief.

It is somewhat curious that although speculative intellectual activity and creative art are unique human characteristics, neither the intellectual nor the artist has ever succeeded in winning for himself a firm and well-recognized place in modern society. If society supports such people at all, it usually does so for an ulterior motive or for some secondary result. Most good chemists do chemistry because they are interested in how molecules are put together. But society supports chemistry because girls like nylon stockings.

The creative artist's situation is slightly different, since the tangible results of his activities are not usually so obvious as ladies' hosiery. Thus the support of creative art by society is, at best, uncertain.

Since the Industrial Revolution, and with the general spread of democratic forms of government, science and art have had more difficulty in being appreciated for themselves. Some European democracies, apparently out of force of habit, continue the royal tradition of official handouts for the performing arts. On the other hand, the overseas democracies that grew up more or less *de novo*, notably the U.S., have essentially neglected the arts and have supported science only because of its practical results.

This preoccupation with practical results reflects a reluctance to admit that science may have something to say about the higher aspects or values of human life. We tend to underplay the role of science as a part of our equipment for an intellectual understanding of man's place in nature, his hopes, his fears, and his sources of satisfaction.

Thus we use science and technology merely to satisfy our needs and support the values that have come down to us from a previous more primitive culture. Insofar as we have succeeded in isolating our thoughts and feelings about the purposes and values of human life from our thinking about the natural world, we have succeeded in preventing man's most effective intellectual device, that is basic research methods, from engaging with man's most persistent problems. There are signs, however, that our single-minded devotion to science as a means for providing ourselves with more and more accessories is beginning to get us into trouble.

The way to get out of trouble may be to make greater use of science to understand man's place in nature. This is not to say that science can by itself provide better answers than others, that it should in fact be "on top" rather than "on tap." In certain areas of increasing importance, however, science can lay at least equal claim to a place at the council table.

Perhaps science's strongest claim to such a place is based on the embarrassing fact that science creates as well as solves problems. A knowledge of science thus becomes essential to understand what some of our most important problems are.

In the 20th century we have run into trouble. The ability of science and technology to solve problems seems curiously intertwined with an ability to create new ones. Some of these new problems may be, in fact, even harder to solve than the old ones.

Improvements in agriculture, in transportation and, more recently, in public health and medicine have temporarily relieved hunger and improved health. These improvements are automatically and inevitably followed, as Malthus foresaw, by an increase in population that tends to consume the increased production. In certain areas of the world, a cyclical situation characterized by relative prosperity interspersed with acute famine has given away to a less dramatic, chronic scarcity where no one ever has quite enough to eat. Furthermore, the rapid application of new technology to agriculture is characterized almost everywhere by displacement of large numbers of people into already overcrowded cities.

Improvements in the technology of medicine and public health have made possible the prolongation of life in circumstances that raise serious doubts about longer life as an absolute and unquestionable value. Although in the advanced and affluent countries science has made the conventional means of subsistence available to almost everyone, it has also created new and very expensive means of subsistence for individuals who formerly could have laid no claim to life at all.

In the best of circumstances it may cost $20 to $25 a day to keep someone alive on an artificial kidney. Many such individuals can doubtless return full value to society, but what about the totally incapacitated bedridden patients who require artificial feeding and nursing care around the clock?

When science has made it possible to prolong almost any life indefinitely, an inevitable limitation of resources will drive us to admit that there is something simple-minded about regarding all lives as equally worth saving at all cost. Limitations on resources, if nothing else, will force us to compare the costs and benefits of saving one man as against another. We may even become sophisticated enough to recognize that death itself carries positive as well as negative values, for science tells us that without death to remove the ill adapted and worn out, both biologic and cultural evolution would slow down to a dangerous degree.

The progress of technology has also changed the nature of war international relations. From about the middle of the 17th century until the beginning of the 19th, war was an increasingly professional activity and attempts were made to isolate civilian populations as much as possible from its effects.

Now the trend is reversed, partly because of the spread of democracy, which gave a new kind of sanction to the idea of the universal draft, but even more because of the invention of weapons of mass destruction. War is thus rapidly returning to its primitive state described in the Declaration of Independence as "an undistinguished destruction of all ages, sexes, and conditions."

The recognition that applied science poses as many new problems as it solves old ones has caused some observers even to question the idea of progress. Others go so far as to question the wisdom of promoting further scientific research at all. Such naive disillusionment is a natural result of overvaluing science for its practical application and undervaluing it as an intellectual activity.

One of the few things that distinguishes man from the lower animals is his ability to contemplate the meaning of his own existence. Even in prehistoric times, such customs as burying food, weapons, and tools along with the dead give evidence of a wish to transcend the boundaries of local space and time. One of the primary preoccupations of the best minds has always been the refining of our knowledge of the shape, size, and motions of the planet on which we live and its place in an ever-widening universe. The nature of the creative and degenerative processes that produce this universe has always fascinated us.

Certainly some of the knowledge so derived will have a practical bearing on earth-bound technology, but this is not what excites most cosmologists, and it's unlikely that it inspires the very substantial public interest in the subject. Astronomy is one science that seems to be loved rather widely for its own sake.

On another level, man has always been interested in the living world. He is captivated by its variety and has speculated endlessly on how it was created. These speculations have led to one of his most spectacular intellectual achievements, the doctrine of evolution.

It turns out, of course, that astronomical and biological discoveries have greatly changed man's attitude toward himself. Some of these changes were initially rather unsettling. The idea that the earth occupies a peripheral rather than a central place in creation was repugnant to theologians and others who wanted to trace man's dignity to some central concern in the mind of God. Similarly, many people found it difficult to accept the humility engendered by an understanding of the doctrine of evolution.

But for most, these discoveries have carried a big plus. Man no longer need be terrified by anything except himself. Although the evidence is overwhelming that he is on very sound ground in fearing himself—at least the problem is one of human scale.

Man once lived in constant fear of the forces of nature that could not be controlled and only uncertainly propitiated. Now, armed with scientific knowledge, man is able to control most of these forces so that among other things, the normal life span has become three score and ten for the great majority. In scientifically progressing countries food supply is not a serious problem.

Man's life can now be what he chooses it to be and not what is determined by outside forces. This in turn has greatly altered his feelings about profound metaphysical and theological questions. Basic science as an intellectual activity is thus a critically important part of man's total intellectual apparatus for resolving his identity crisis. Of course, the catch to man's increasing power is that he does have to make more choices, and he is only slowly coming to realize how many choices he is going to have to make.

In a real sense, men have to decide now how many people should inhabit the earth. As our ability to postpone death increases, we must ask whether death is the unmitigated evil we used to think it was, and inquire into its function as a facilitator if not the actual driving force of biological and social evolution.

As we assume more responsibility for our own lives, we have become suddenly aware of the responsibility for the natural environment in which we live. It seems inevitable that our lakes and water courses will have to be used partly as sources of food and recreation, partly as sewers, partly as cooling water for electric turbines, and partly as sources of water for domestic and industrial use. What is the proper mix in each case?

Man can no longer leave this kind of decision to the impersonal forces of nature. We are beginning to see that each decision depends on a combination of intellectual analysis and esthetic and moral sensitivity. Further development of the scientific method and of intellectual analysis is absolutely essential for describing the choices and the respective price tags. The final choice among the alternatives, however, continues to be largely a matter of feeling.

For the past century, many scientists on the one hand and many humanists on the other have pretended that science deals with man's external life while the humanities and arts are concerned with the inner or spiritual man.

This separation may have been all right so long as an engineer was somebody who made automobiles when everyone was sure that more automobiles were what they wanted; or a doctor was somebody one called in when one was ill and wanted to get well. Now, however, an engineer is becoming a person who says, "all right you can have more automobiles, but if you do you will have the following kinds of traffic and pollution problems in your cities;" and a doctor is called not only to help a couple have a child but to determine what kind of a child it shall be. Under these circumstances, the separation of the intellectual world into a science involved only with external things and the humanities dealing with internal and spiritual ones is no longer tolerable. We must return to an earlier time when science was pursued at least in part under the heading of natural philosophy as one of the means for understanding the nature of man and his place in nature.

Increasingly, the forces that shape man's life are becoming matters of conscious choice. Although intellect must probably always take second place to esthetic feeling at the moment of ultimate choice, vigorous intellectual analysis of the possibilities is an essential prerequisite to the final judgment. For a value judgment made in ignorance of all the possibilities is no judgment at all; it is simply a groping in the dark.

During the past year we have been reminded that society will not give science or any other intellectual activity all the support it needs. Increasingly we are being asked to justify our work; and more and more we see the justification asked for in specific and limited terms. How is radio astronomy related to national defense? How is enzyme chemistry related to a cure for cancer?

It is not easy to weigh the need for a new and more powerful accelerator against the need for better housing and better schools for the depressed populations of the cities. But this does not mean that immediately foreseeable tangible results are the only things to be weighed in the choice.

As we prepare the case for the support of science even in troubled times, we should not be embarrassed to cite the role of science in giving man a clearer picture of himself and his place in nature, and in providing the intellectual base for the crucial choices he must make in the future.

BASIC RESEARCH'S ROLE IN TEACHING

FRANK H. WESTHEIMER, *Harvard University*

Research in universities and colleges provides the competence and enthusiasm that make teaching live. It brings interest to undergraduate lecturing and in and of itself constitutes the core of teaching in graduate school. Yet academic research is attacked today; it is described by slogans such as "the flight from teaching" and cited as the cause of the rebellions in our universities; research is alleged to result in the victimization of students by a faculty that doesn't care about the undergraduates.

Actually, the situation is quite the opposite, at least for the sciences. Teaching is better today than it has ever been, and it is best where research thrives. Although student ferment has upset many campuses, the cause of the trouble is not too much research. The difficulties may lie, in part, in having too little research and having too few students personally engaged in discovery.

During the uproar at Columbia University in May 1968, few rioters were students of engineering, medicine, law, journalism, or the physical sciences. This may suggest only that scientists are committed to achievement within "the system." It may also lead to a conclusion that, in science, the university is fulfilling one of its primary functions: engaging students' imagination and enthusiasm for intellectual problems.

One cannot decide whether teaching is good or bad until he defines his terms, until he establishes his value judgments. Dr. James Killian, chairman of MIT Corp., offers one set of value judgments: "The purpose of teaching in the modern university is not merely to fill the student's mind with known facts, theories, and modes of thought; it is also, and more important, to stimulate him to teach himself, to learn by teaching others, to think creatively, to want to seek answers to questions as yet unexplored, and to learn the arts of doing so."

Dr. Killian concludes, "I know of no better way to do this than to give [the student] the opportunity to work with and under faculty members who themselves are engaged in seeking answers and who can, in consequence, impart a sense of intellectual adventure."

An alternative objective of education is that of transmitting to students the accumulated knowledge of the past. Science is changing so fast, however, that past knowledge is soon inadequate.

If we regard human beings as inherently curious, then research is probably the tool for hooking them for life on the "intellectual adventure." We can hardly hope to offer everyone the opportunity to carry out original research. But some of what is offered to students can be presented in problem form so that they can at least participate in the excitement of rediscovery. Teaching of this sort is most likely to come from those who solve problems themselves, or have once done so, and therefore know the process.

Many critics point out that university faculties are selected for their excellence in research. It is perhaps because of this increased emphasis on research as a prerequisite for teaching that undergraduate teaching—the presumed victim for whom all the tears are shed—is in the best shape ever.

Riesman and Jencks, in "The Academic Revolution," wrote, "If we use a strictly academic yardstick, it seems clear that today's A.B.'s know more in absolute terms than their predecessors in any earlier era. . . . This is particularly clear in science and mathematics. . . ." Some measure of the performance of today's students relative to those of the past can be gauged from questions from freshman chemistry examinations at Harvard.

- 1868: "Write the symbols of Water; Sodic Oxide;

Sodic Hydrate; Nitric Acid; Sodic Nitrate; Sodic Chloride; Sodic Bromide."

- 1918: "By equation show what happens when aluminum sulfate is treated with an excess of sodium hydroxide."
- 1968: "The standard electrode potentials for the reactions that take place in the lead storage cells are:

$$Pb + SO_4^{2-} = PbSO_4 + 2e \quad E^\circ = -0.356V$$

$$PbSO_4 + 2H_2O = PbO_2 + SO_4^{2-} + 4H^+ + 2e \quad E^\circ = 1.685V$$

What is the maximum energy in calories that can be obtained from 104 g of lead and 120 g of PbO_2?"

These questions show not only the increase in the factual knowledge expected of students today but, more significantly, the expectation that the students can use that knowledge.

Research and teaching are closely related; for graduate students the functions are essentially identical. Graduate student research is the core of our advanced teaching programs. It shows the student, through detailed and specific examples, the excitement of answering new questions. Best of all, it provides lessons in seeking new problems. It is this spirit of inquiry that constitutes the best liberal education.

The direct impact of research on teaching is not restricted to graduate students. In the sciences, and in chemistry in particular, many undergraduates participate. In 1963–64, about 1500 undergraduates, approximately one third of the chemistry A.B.'s, were engaged in research in chemistry during the academic year, and nearly as many—although there is some overlap—were engaged in research during the summer.

Although a sizable fraction of chemistry students are engaged in the discipline and excitement of research, most college students are only exposed to what others have learned or thought. The fortunate undergraduates in research may not accomplish much on the average in terms of the advancement of science, but they will have learned how the human race goes about learning.

Great scientists teach in undergraduate courses, and teach with enthusiasm. Enrico Fermi, when he was director of the Institute of Nuclear Studies at the University of Chicago, *insisted* on the privilege of teaching freshman physics. Among the most admired and original undergraduate science courses in America have been Richard Feynman's freshman physics and Linus Pauling's freshman chemistry at Caltech and George Wald's general education course, based on biology, at Harvard.

It is true that a few scientists retreat to their laboratories and remain aloof from undergraduates. Such men are excoriated by the critics of university research. However, the universities may fulfill their purposes best by allowing a few rare individuals to restrict their teaching duties to initiating graduate students in original research.

It should be noted, however, that research professorships are rare. A survey of 14 major private and state universities showed only a few such positions. The absolute number is difficult to arrive at, but the survey indicated the equivalent of about six to eight full-time research professorships among the 500 members of the chemistry departments of the universities surveyed.

A list of the instructors for the 1967–68 year in this same group of universities for all courses in elementary organic chemistry—including those for chemistry majors, for medical students, and the like—includes many full professors and some of the leading organic chemists. Obviously elementary organic chemistry in these universities is well served. Similar data could be obtained for other areas. For example, all undergraduate chemistry courses at Harvard are regularly taught by full professors.

One of the most persistent accusations against faculty is that they shirk their teaching responsibilities in favor of travel, consulting, and the like. The point is easily challenged. For about 30 of the professors who lecture in elementary organic chemistry at the universities referred to, the average number of undergraduate lectures missed, for any reason but illness, during 1967 was fewer than two per lecturer; half the lecturers did not miss any.

It appears then that the best research scientists in the universities lecture to undergraduates as well as graduates. But the question may still be asked: Do they give good undergraduate courses?

Student opinion may be used as a criterion, although one doesn't know what the students' value systems are. The rationale for considering student opinion is presumably that the best teaching is that which best engages student interest and enthusiasm.

If student opinion is acceptable, then a consideration of preferences of the brightest students would be useful. They clamor to attend Caltech, Stanford, Yale, Wisconsin, Berkeley, and Michigan, and a few small colleges, such as Reed, Oberlin, and Haverford. The universities they choose are the leaders in research, and at least the scientists in the small prestige colleges are generally engaged in research.

The students' selections do not mean that the universities meet all their expectations. Yet the students' choices do mean that they are convinced that the universities will give them a better education than they could obtain elsewhere. Somehow good teaching and research seem to go together.

Another measure of research chemists' contribution to teaching is the number and quality of the books they write. A professor can lecture to only a few hundred students; if he writes a fine textbook he can reach tens of thousands.

Another activity of university faculties that demonstrates interest in teaching is curriculum revision for secondary schools. The "new math" originated when a group of university mathematicians got together to bring the past century's revolution in mathematics from university research to the schools. Numerous university physicists have participated in creating a new high school physics curriculum. New high school chemistry and biology texts have been developed with the cooperation of university research scholars.

All of this is evidence that research scholars in chemistry constitute the heart of the teaching enterprise—through their undergraduate and graduate courses, through books they write and the curriculum reform they initiate, and through the research they conduct with graduate and undergraduate students.

We have many opportunities to improve teaching in chemistry. First, we must continue to update our courses. The advance of science demands that scientists run fast to stay in the same place, and research is almost certainly the best way for an individual to maintain his interest in new developments. To the extent that the scientific community adopts Dr. Killian's value judgments, we should incorporate more problem-solving into teaching, and let students learn through the excitement of rediscovery.

Some improvements will come by bringing students into research as soon as practical, but modern research on teaching methods suggests that there may be other ways to progress. One exciting avenue rests on computer-assisted problem-solving examinations.

We also have the opportunity to make substantial improvements in our laboratory courses. Perhaps because we have not completely analyzed what we want students to achieve in the laboratory, we do not agree on the design of lively and educational experiments.

Most laboratory instruction in universities is carried out by graduate students. Although this system might be expected to benefit teacher and learner alike, the graduate students often feel exploited and the undergraduates often feel cheated. We may be missing an opportunity here to connect research and teaching and to upgrade our performance.

Universities face an interrelated set of crises today. We need to engage the students more in learning and we need to bring more problem-solving and research to teaching. Nevertheless in science, the strength of our system derives in large measure from the enthusiastic, occasionally inspired, and usually skilled teaching of those who do research.

INDUSTRIAL BASIC RESEARCH

ARTHUR M. BUECHE, *General Electric Co.*

How much money and effort should a profit-oriented industrial organization devote to the search for new knowledge about nature? Having determined an appropriate level for this activity, how does a company go about doing the job?

If "searching for new knowledge about nature" is a sufficient definition of scientific research, it is merely a matter of semantics whether the search is described as basic or applied. It's what is learned that matters.

Although the motivations of the scientist and of his sponsor can be quite different, in most cases the motivations are somewhat similar and often identical. Even so there is no harm in a situation in which the scientist is pursuing a strictly scholarly course in an area in which his sponsor believes there is a reasonable chance of applying whatever is learned.

It could be put this way: "One man's ivory tower is another man's workshop." Or: "Basicness is in the eye of the beholder."

Thus, the only legitimate distinction that can be made between general types of technical work is that which differentiates seeking new knowledge from applying what is known.

This is a distinction between types of work, not necessarily between types of people. Researchers frequently get involved at least part-time in development, and engineers sometimes must do their own scholarly digging for new information.

New ideas are the important thing, and new ideas are not the exclusive province of either the researcher or the development man. Getting good new technical ideas is the ultimate objective of all industrial research and development. Research is therefore performed because new knowledge can be the seed for a good new idea, or the basis for converting an old idea into a good one.

Obviously, the answer to the question of how much money and effort should be devoted to research depends on the company. It is not a matter of size or diversity, but rather one of overall objectives and managerial approach. A multibillion-dollar retail sales chain may need less scientific research than a million-dollar newcomer in a specialized area of electronics. A highly diversified conglomerate seeking to grow by acquisition needs far less scientific support than a company seeking to grow from within. The company locked in a fierce battle with competition needs more research than those—if they still exist anywhere—that feel satisfied and safe.

An old-line business that has been doing everything the same way for years may really need a scientific research team more than a new business in an area where new knowledge is being generated by world science far faster than anyone can properly digest it.

The chief executive officer seeking the right level for his scientific research program must ask himself other questions: How many good new ideas do I really need? Is my business the kind where the new ideas must be technical ideas? Are my existing products improving fast enough to remain competitive? Do I want to grow by building on our own new ideas or by buying up someone else's? Am I able to use all the good ideas my people are already generating? Am I getting as many good new ideas as my competitors? How long can I really stay in business without new ideas?

If a chief executive officer is convinced that getting new knowledge is an excellent way to ensure obtaining new ideas, he will recognize that the support of scientific research is an absolute business necessity. The optimum level of such activity is reached when the flow of good technical ideas is sufficient for the organization to gain and maintain the objectives it has set for itself.

Even this "scientific approach" to the establishment of appropriate levels for research effort in industry is not foolproof. It is not uncommon, even in the most sophisticated and professionally managed organization, to have a research budget which is half as big as the research director believes it should be and twice as big as the comptroller believes it should be.

The fact remains that these decisions are made and that many companies do recognize that research is an essential function of business. U.S. industries spent an estimated $3 billion in 1968—of their own money, out of profits—on the search for new knowledge about nature.

How, then, does research help industry? Specifically, it provides knowledge to spark ideas to improve or develop products and services that customers need and want and will pay for, permitting the company to grow and make a profit.

Having decided that it should avail itself of this opportunity, how does an industrial concern actually go about the pursuit of scientific scholarship? How does it get the good people it needs?

First of all, the company must decide what general areas of science are important to its line of business, present and future. In highly diversified companies, virtually any discipline may be useful, but there is always the problem of emphasis and balance.

The next step is to go to the universities, the most important supplier. The ultimate objective, of course, is to be in a position each year to bid on the cream of each year's crop of graduates—from among those whose work and theses have made it apparent that they are interested in matters that relate to the company's interest.

There is no absolute guarantee that the top graduate student will become the truly creative professional scientist. However, finding the few truly creative giants among the host of good-but-not-great technical people is something of a lottery, and experience shows that the odds are somewhat better among those who have attained an advanced degree and have been superior students.

There was a time when industry enticed their top candidates by the simple process of waving money in front of them. The situation is now much more competitive, however, and not merely among the industrial laboratories. The ability of the universities to keep their best for themselves and the attractions of exciting government programs have made life much more difficult for the industrial recruiter. But he would be the first to admit that, all things considered, this competition is a good thing for everybody.

The new young scientist arriving at an industrial research laboratory deserves a chance, at the beginning, to prove that he can do research, on his own and in his own way, that will be important and useful.

The word "useful" will be a red warning flag to some people. It connotes visions of laboratory notebooks with profit-and-loss figures in the lower right-hand corner of each page. That's not what is implied. The research will be useful to the company if it is important as scientific research.

The majority of nonproductive industrial research that goes on today is not nonproductive because it fails to solve a production problem, or fails to result in a profitable new product, or because it comes up with something that the sponsor is either too stupid or too stubborn to use. Instead, the majority of the nonproductive industrial research is useless for the same reason that the same kind of work is useless if it's done in a university or a government laboratory, or anywhere else. It is useless because it is not important to science.

Simply stated, if research isn't important and contributing usefully to the furtherance of science itself, then it's pointless to worry about whether it's good for the company, or good for the nation, or good for people.

If the research is good science, then chances are it will pay off—someday, in some way—for the sponsor, and the nation, and the human race.

Much of the concern of Congress, the general public, and company shareowners and boards of directors over the matter of "basic research" is not the result of "useless science." It is instead the result of so-called technical work that is useless because it's not, from a professional point of view, making any contribution to the profession.

Planning research that is useful—important as science—calls for a special kind of judgment and objectivity. There is no reason to believe that all graduate Ph.D.'s have these abilities, or ever will have. The perpetual shortage of really good research managers is ample evidence of the problem.

Actually, in an industrial laboratory, it is far easier to find a man who thinks he knows what kind of research is good for the company than it is to find one who knows what is really good research. The one doing the latter job well, however, can't help doing, in the long run, a better job of the former.

In any event, the new young scientist on the payroll deserves a chance to show that he has the judgment and objectivity to do good work on his own. If he does have these qualities, then the company may give him his head and hope that he'll also develop that spark of creative genius that is found among perhaps one out of every 10 Ph.D.'s.

What happens when one of these self-starters develops new interests and his curiosity leads him into fields having no foreseeable connection with the company's business? In spite of the talk about the waywardness of "truly pure" researchers, the fact is that most scientists, and especially the best ones, are very anxious to see something practical and useful result from their work. They direct their interests and enthusiasms accordingly. Their own enthusiasm is often the greatest motivator for application of the results of their work. Some even complain about how slowly the development department does its part of the job.

Thus the problem of the man doing really good work in the wrong area doesn't arise very often. When it does almost any research director will recognize it as an opportunity to simultaneously serve the sensitivities of the scientist and diversify the interests of the sponsor into new areas.

As long as reasonable balance is maintained, it is absolutely essential for an industrial research enterprise to encourage blue-sky exploration both inside and outside the technical areas of obvious interest to the business. There are numerous examples of blue-sky research leading unexpectedly to new products and services. It's not as likely to do so as research deliberately done with these ends in view, although there are those who will argue about that, too.

More likely, a researcher eventually needs or wants guidance in deciding what kind of work he should be doing. This is no admission of lack of creativity or ability to make major contributions to science. It follows then that the largest share of research people are working not only within the bounds of mutually agreeable fields of endeavor, but also under varying degrees of direction.

General Electric's "directed" technical people include everyone from guided loners to members of fairly large teams assembled to accomplish major missions for the company or for the government. This means that many of the scientists are, simultaneously, doing research and development. In the General Electric Research and Development Center, with a total of more than 600 scientists and engineers, a substantial fraction of the people do genuine research most of the time. That is, they spend more time searching for something new than applying something known.

The so-called "directed" people in industry are just as dedicated as the rare self-directed scientists. They have the same curiosity and pride in their work, and they use the same method of discovery that Newton is said to have described to the lady who asked him how he really came to discover the law of gravitation. His reply was: "By thinking about it constantly, madam."

Considering that research does help industry by providing knowledge to spark ideas to improve or develop products and services that customers need and want and will pay for, thereby permitting the industry to grow and make a profit, there are those who might describe this as a crass commercial response. This is especially true since one also asks, "What does research, including industrial research, do for the human mind and the human spirit?"

One might reply that the value, real or aesthetic, of new knowledge is neither diminished nor increased by the source of the supporting funds, whether spent by industry directly or funneled through the government to its own or university laboratories.

Industry, just by being businesslike and profitable, fulfills a social obligation as the chief creator of the capital required to solve many of mankind's problems. People in industry recognize that they must continue to do their traditional job, but at the same time must make a special effort to adapt products and services, and ways of doing business, so that they will directly help fulfill the now unfulfilled expectations of people who, increasingly, know what they really want and need.

For this latter reason especially, industry needs new ideas—new ideas triggered by new knowledge in all areas. It needs the kind of people who can generate the new knowledge and get the good ideas. It can provide opportunities for such people only if it demonstrates a sincere desire to support scholarly pursuit, including good science. After that, its job is to use its special skills to put ideas to work for mankind.

FEDERAL GOVERNMENT'S ROLE

DONALD F. HORNIG, *Eastman Kodak Co.*

The executive branch of the Federal Government and the Congress would like to know what research is all about, and they would like to know what they are spending the country's money for. By and large the decision-makers in the federal establishment don't really have a clear idea of what a researcher actually does and why whatever he does is vitally important to the country.

The problem today, and this is felt greatest at the federal level, is that basic research isn't properly appreciated, isn't properly understood and, in some sense, may need a clearer exposition, both in the academic and the industrial areas, as well as on the national scene as a whole.

The recognition of this problem derives from the continuing discussion of such questions as the utility of basic research, research for its own sake, and whether basic research in universities detracts from teaching. The problem also reflects allegations from some industrial segments that federal funds for research in the universities have diverted good people from the useful application of their talents. These discussions go on constantly within the Federal Government and there is a running debate on the proper place of basic research.

It is very difficult to define basic research. The reason is clear; basic research is in the eye of the beholder. Research is the systematic attempt to obtain new knowledge, and basic research presumably means that which is not directly motivated by a probable application. However, an activity cannot be defined by its motivation.

Moreover, basic research in one field may be applied research in another. For example, research on carburetors may well be basic research in the automotive field but is surely considered applied when viewed from the standpoint of some other branches of science. Nevertheless, there seems to be a general idea of what is meant by basic research, at least when used in chemistry.

The problem is not that the modern world doesn't appreciate the need for intellectual stimulation of society by basic research, for broadening our horizons, for increasing the options open to society, and for enlarging the stage on which the human drama is enacted. This is widely understood and society has generally given wide approval to basic research.

Laymen are definitely interested in science. There has perhaps never been a time in history when as many school children made electromagnets and did elementary chemical experiments. Ten-year-old boys debate why, to make one rocket catch up with another in orbit, you slow down the one that's behind. (This is the proper strategy, too.)

So there is a broad general interest in science and in basic research. It is also true that there is a general public conviction regarding the utility of research. What the discussion is about, however, is short-term *vs.* long-term utility rather than utility or nonutility. The sciences have successfully conveyed the idea that an increased understanding of the globe on which we live will some day, some place, and in some time, result in man's being able to do something to increase his stature as a civilized human being.

Until about 20 years ago the Federal Government had little involvement in research, and none at all in basic research. Today this country spends about $3 billion a year on basic research, of which about $2.5 billion originates with the Federal Government. In other words, about 80% of the basic research support in this country is provided by tax dollars. For this reason, the attitudes of the Federal Government and the public are critical in determining research support now and in the future.

There is no point in reacting as if the problem were to get the Federal Government interested in the support of basic research. This problem was settled in the late 1940's when eloquent cases were made that, given the broad spectrum of activities that are science dependent, there is a direct governmental interest in maintaining a healthy and viable science establishment. One major report, made during the Truman Administration, looked forward a decade or two to foresee a federal support level of $300 to 400 million.

The Federal Government is therefore providing support six to seven times greater than what was the ideal of the late 1940's. Presumably the goals have been revised upward, so what is relevant now is what the scientific community wants in addition to what it already has.

What is being done in basic research right now? About 60% ($1.5 billion) of the total federal basic research expenditures is spent on basic research in universities. Approximately 30% ($750 million) of the federal money is spent within the federal establishment and in government laboratories. The remaining 10% ($250 million) is spent on basic research in industry (nonuniversity and nongovernmental establishments).

Another way of looking at federal basic research support is to ask who in the government is spending the money? To whom do the arguments about the virtues of basic research have to be made, and against what background?

One of the major performers is the National Institutes of Health. Its basic research is health oriented, and whatever the reasons of those who perform the research, the national argument for support of NIH research is health and the present and prospective improvement in the nation's health.

The National Aeronautics and Space Administration supports about the same amount of basic research as NIH. One of the problems in basic research is encountered at NASA. Chemists can get very enthusiastic about support for basic research in chemistry, but chemists are apt to say that buying rockets is not basic research.

The exploration of space, nevertheless, is basic research, since it is surely one of the great enterprises in expanding the human imagination, and the purchase of rockets to explore space is therefore a cost of doing basic research.

The Atomic Energy Commission's basic research centers on the dream of the peaceful uses of atomic energy. Also included in its programs is the special area of high-energy physics, which has very little to do with anything practical and is very basic indeed.

The Department of Defense, another big science budget area, conceived the idea that basic research, while it may have nothing whatsoever to do with the immediate problems of defense, was a national asset that had to be encouraged for the long-run security of the nation.

Last but not least is the National Science Foundation, which is the only one on the list whose mission is to support basic research and education in science, per se. NSF spent about $250 million directly on research out of a total expenditure of $456 million in 1968.

There are obviously many different kinds of basic research. Those that have received the most public attention (and the most criticism) are the so-called big sciences. NASA's space program is a conspicuous example. Likewise, high-energy physics comes in for criticism.

Radio astronomy is one of the most exciting new fields of basic research that is also big and costly. In the past 20 years the bounds of the known universe have been moved out more than 10 times. Today, arrays of radiotelescopes study previously unknown objects, such as quasars and pulsars. Radiotelescopes on opposite sides of the globe are coupled together and synchronized to a fraction of a microsecond so that they can be used as if they were a single interferometer with a baseline the size of the earth. This is scientifically very exciting, but it is also very expensive.

The science of the earth's atmosphere is another case. Longer range weather forecasting, for example, will require big and expensive worldwide observation systems, since a single disturbance circles the globe in less than two weeks. There is no way of doing this by local observation alone. Such systems will depend on satellites either to make observations or as a communications device.

The science of the solid earth has also entered a new era, and oceanography has recently become a big and now costly science.

When members of the scientific community discuss basic research *vs.* nonscientific projects they find it easy to argue for more research. However, when the scientific community begins to argue with itself—space *vs.* high-energy physics or perhaps *vs.* chemistry—the pure intellectual excitement of another scientist's field doesn't seem to have any more appeal than it does for the nonscientific community.

For example, there seems to be considerable skepticism among many scientists regarding the U.S. space effort. This program is revealing the chemical composition and the closeup texture of the moon. It has shown that Mars has craters like the moon and an atmosphere with a density only a hundredth of what was thought 20 years ago. Nevertheless the mood persists that while this may be exciting to chemical scientists, they say, "Why don't we put the money into chemistry?"

Nonscientists feel the same way about roads and social programs. In their view everything said about basic research is right, but other things are needed more and needed sooner.

What then does the future hold? Up to now growth has been based on regarding basic research as an integral part of many other activities. Cogent arguments have been made that in any future security situation the total scientific strength of the country will be relevant just as the industrial strength of the country is relevant.

Furthermore, basic research is an integral component of education, and education is, in turn, an important part of research.

In a sense, then, scientific development in the U.S. has ridden on the coat tails of other objectives. This has the consequence that the case for basic research has to be made separately with respect to each of the nation's major objectives.

Some people seem to be worried that we are on the verge of national collapse from the standpoint of research support. It must be realized, though, that the U.S. spends half again as much on basic research, as a percentage of its gross national product, as any other country in the world. Since the per capita GNP of the U.S. is 40% greater than that of any European country, this equates to saying that the U.S. spends about twice as much per capita on basic research as any European country.

The American effort is very large by any standard. There are important and cogent reasons why more can and should be done, but one must start from the realization that the U.S. is doing much more than any other country in the world, including the Soviet Union.

What the scientists have not succeeded in doing, and this shows up in the problems of NSF, is to create an adequate constituency in the country for research. There is, of course, a constituency. After all, Congress is appropriating a substantial NSF budget. However, there is not an adequate constituency built around a clear understanding of the role of research and education. That is one of the tasks of the scientific community.

Scientists are going to have to make a better case for basic research, and they are going to have to make it from a number of points of view. Some sense of significance, not just practical utility, but intellectual consequence, must be conveyed to the public. This must be done continuously and to many audiences.

In particular, there are not only the many federal agencies to be addressed, but there are about 34 Congressional subcommittees that concern themselves with the problem of basic research. Each has to be addressed in one way or another and educated constantly, not just in periods of crisis.

For example, state universities do an extraordinarily good job of keeping their own state legislatures informed. Hardly anyone at that level, however, even discusses with his Congressman what goes on at the university and how it is affected by federal funds.

What is the present problem? The level of support for basic research has increased by about 40% in this country since 1964, but the rate has been level since 1967. The level of expenditures in 1968 will, as part of the general budgetary tightening, likely be down a little.

It is difficult to say whether this cutback in growth reflects a special problem for science and scientific research, or whether it should be interpreted simply as part of the Congressional reaction to a tight budget situation. The budget cuts have been deeper in research and development than in other areas, but that perhaps reflects a feeling in Congress that research is a deferrable item.

A more serious symptom, perhaps, is that NSF, which is *the* agency tied to basic research per se, has had its obligational authority (not its current expenditures) cut 20% by Congress. This is a serious, substantial matter since it reflects a general lack of understanding of the purposes to be served by NSF.

The scientific community cannot continue its discussion with Congress and the executive agencies solely in terms of funding levels. Instead, scientists have to recognize that there are better and worse ways to spend public money, and must therefore help to make choices of what is good and significant in basic research.

Scientists must also take a hard look at the federal science organization. The concept of a larger department of science, perhaps at the Cabinet level, with NSF as a core, should be re-examined.

In trying to get comparable standards of judgment and in setting priorities among such varied things as space, high-energy physics, and chemistry, there is no question that the pluralism of the government, the number of agencies involved, the number of Congressional committees and subcommittees, all lead to a certain amount of chaos. The question of a large science agency or a department of science must then be reopened, although it would still seem unwise to concentrate all science activities in a central agency.

The realization that science is part of everything has been a strength of the American establishment. Therefore, those research activities that are integral to a department's mission or form the basis for its future should be left where they are. On the other hand, the possibility of a large department, particularly for basic research and education, should be examined.

In making the case for basic research, scientists should not look at the question of fair shares, nor endorse busy work and trivia but, rather, should examine and bring into focus the unmet opportunities and the challenges of the future.

Appendix

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